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PRAGMATICS AND INTENSION

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## I

Carnap, in an endeavour to meet Quine's objections to his definition of the concepts of 'synonymy' and 'analyticity', has tried to set up a method of testing intensions pragmatically.<sup>1</sup> For Carnap, not only can we ascertain empirically the extension of a predicate, *i.e.* the objects it denotes, but we can also by empirical methods determine its intension. He maintains that we can state a behaviouristic procedure for the determination of conceptual meaning. As he puts it, "the assignment of an intension is an empirical hypothesis which, like any other hypothesis in linguistics, can be tested by observations of language behaviour".<sup>2</sup> We can, for example, enumerate the possible meanings of a word (in the form of descriptions), asking the individual to select the one he thinks to be most appropriate. In this way Carnap believes we can in some measure determine the precise meanings given by individuals to the words they use.

Carnap, however, still perpetuates the sharp distinction between analytic and synthetic, overlooking thereby that this is just the point under discussion. Quine's criticisms do not for him in any way concern the formal correctness of pure semantics, as Carnap regards semantic concepts as having some sort of independence and as being fruitful on their own account. Quine, he assumes, is rather doubting whether there are any clear and fruitful corresponding pragmatic concepts which could serve as explicanda for the semantic concepts, since "without this pragmatical substructure, the semantical intension concepts, even if formally correct, are arbitrary and without purpose".<sup>3</sup> Quine's position, however, would seem to be that, whatever significance semantic concepts have, they can (or ought) always be formulated

<sup>1</sup> R. Carnap, *Meaning and Synonymy in Natural Languages*, Philosophical Studies, April, 1955.

<sup>2</sup> *Ibid.*, p. 37.

<sup>3</sup> *Ibid.*, p. 35.

behaviourally. On such a view, pure semantics cannot be sharply differentiated from pragmatics. He is thus not simply asking, as Carnap implies, for the production of the corresponding pragmatic concepts.

Carnap's contention that there is an empirical method for testing intensions no doubt stems from his view that a concept (or an intension) is something objective.<sup>4</sup> He tells us, for example, that the intension of the word *blau* in German is the property of being blue. Carnap seems to assume that over and above the objects in the real world there is a system of such possible properties which gives meaning to our language. He tells us that, unlike the case of the extension of a predicate, there are more than one and possibly infinitely many properties which the same predicate can express. To illustrate this he postulates two linguists unfamiliar with German who are studying the language habits of a German-speaking person. Carnap argues that though they may come to agree that a certain predicate denotes a particular object for that person, whom we may call Karl, they may still disagree as to the precise meaning it has for him. Thus the assignment of an intension to a predicate becomes for the two linguists an empirical hypothesis. It has this character since (a) they cannot know precisely what intension Karl has in mind when he applies this predicate, and (b) the intensions postulated are regarded as selections from such a system of possibilities.

Carnap, who regards language on a pragmatic level as a system of dispositions for the use of expressions, defines the ability of an individual to apply the right predicate to the right property (*i.e.* what it means for a predicate to have an intension) as a "disposition of ascribing the predicate '*Q*' to any object *y* if and only if *y* has the property *F*".<sup>5</sup> (where *F* is an observable property). However, a strict univocal relationship of this sort is found only in logic and not in our everyday language. The similarity of the phrase 'if and only if' as it occurs in this type of statement, which implicitly refers to an empirical subject-matter, to its use in such a formula as '*p* ≡ *q*' is deceptive. By its use Carnap seems to want to transform an empirical relationship which only holds with a degree of probability into a formal logical one.

Further, this type of definition assumes, for example, that, if a blue object is presented to a subject (whose current language is English), he will tend to respond with the word 'blue'. We seem

<sup>4</sup>The word 'concept' is used by Carnap as a convenient common designation for properties, relations and functions. He emphasises that it does not refer to terms, *i.e.* words or phrases, but to their meanings, and it does not refer to the actual mental characteristics of conceiving but to something objective.

<sup>5</sup>Ibid., p. 42.

to deal with a conditioned reflex theory of how predicates become attached to properties. There is, however, no such necessary restriction on the kind of object which can cause a particular verbal response to occur. It will tend to vary with the experience of each individual, which is usually radically different from the standardised environment of the laboratory animal. Further, the responses of a poet or a lunatic may show still more striking divergencies. This becomes much more evident when we deal with words referring to complex states of affairs, rather than simple colours. As Carnap seems to recognise himself, when he talks about "intensional vagueness", an expression is normally used not in a sharp and precise manner, but over an established range of situations. Nevertheless, he does seem to assume that we have clear cases of objects having properties, and that "intensional vagueness" arises from an inadequate understanding of them on our part.

When, on the other hand, Carnap speaks of the two linguists in their study of Karl's linguistic behaviour reaching complete agreement as to the extension of a given predicate, say *blau*, he seems to slur over the complexity of this process of reaching agreement. Both the German and English languages have a reference to a similar type of Western culture which acts as a common ground between them. As Quine points out, the first moves in understanding a strange language are at bottom a matter of exploiting the overlap of our cultures. If we were dealing with the language of some primitive tribe with bizarre customs, or with beings from outer space equipped with different types of sense-organs, agreement might not be so easy. But in all cases agreement would be reached on the assumption (except perhaps in the case of space-beings) that we were both referring to a common world of objects. It is this among other things which helps us to set up a correspondence between *blau* and 'blue'. The class of objects to which Karl applies *blau* tends to coincide with the class of objects to which we apply 'blue'.

Further, the linguist's interpretation of Karl's language behaviour is dependent upon the assumption that he is not dealing with isolated predicates, but with predicates which are systematically related together. Without some acquaintance with other predicates of the language being studied, the linguist would be unable to determine the precise extension of a single predicate. Indeed, to do this he would have to be acquainted with a substantial part of the language under interpretation. Something similar occurs in the genetic acquisition of a language. Children when they learn a language do so largely through imitating the complex conversation of adults, and not the simplified predicate

form exhibited in the Carnapian sentence. Further, this emphasis on the predicate as one of the key notions of language seems to be more a peculiarity of some Indo-European languages than of language in general. Hence, even before the precise nature of the unknown language has been determined, Carnap assumes that it will have a fairly close resemblance to the syntactical structure of the linguists' own language, presumably English, an assumption which no self-respecting philologist could afford to make. This point is overlooked by Carnap since the unknown language is in this case German, which is in fact Indo-European. But he is, however, not justified in arguing from a special case to language in general. Carnap's whole approach to this question assumes something like a general universal language of predicates, of which the particular natural languages are to be regarded as instances, or, putting it in its traditional form, he assumes a common universe of thought. Carnap would certainly find himself in difficulties if he were investigating a language in which the meaning of the expressions changed from context to context. This, we are told, is the case with some of the more exotic languages spoken, e.g., in the Pacific islands. Malinowski, who studied these languages, was therefore led to stress the instrumental character of a language in contrast to its designative function.

## II

For Carnap, then, the intension of a predicate will include not only actual but also all possible cases. Thus he says, "The intension of a predicate may be defined as its range, which comprehends those possible kinds of objects for which the predicate holds".<sup>6</sup> We may apparently determine this range by starting extensionally from some given specimen denoted by the predicate, and by suitable questioning of the subject find out what variations in shape, size and colour he will admit and still apply the same predicate. Having in this way determined the intension of a predicate, we can proceed to define the notion of 'analyticity' in terms of it (intension seems here to be defined in terms of the logically possible) as follows: "A sentence is *analytic* in  $L$  for  $X$  at  $t$  if its intension (or range or truth-condition) in  $L$  for  $X$  at  $t$  comprehends all possible cases".<sup>7</sup> In other words, a sentence is analytic in a language  $L$  at a particular time  $t$  for a subject  $X$  if it is true solely in virtue of its cognitive meaning, which seems to be taken as self-evident. Synonymy is defined on similar lines: "Two expressions are *synonymous* in the language  $L$  for  $X$  at

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<sup>6</sup> *Ibid.*, p. 39.

<sup>7</sup> *Ibid.*, p. 42.

time  $t$  if they have the same intension in  $L$  for  $X$  at  $t$ .<sup>7</sup> In other words, they are synonymous if they have the same cognitive meaning.

Carnap then believes that 'analyticity' and 'synonymy' as pragmatic concepts, defined in terms of linguistic behaviour, provide a practical justification for the corresponding concepts of pure semantics. However, Carnap's account would seem to be based rather on abstract logical models than on first-hand empirical linguistic investigations. Carnap presents us with a contemplative picture of knowledge in which the 'subject' is in some way assumed to be directly aware of intensions, conceived of as ranges of actual and possible properties, which he can report on when suitably interrogated. Further, the attempt to justify 'analyticity' and 'synonymy' on the pragmatic level by an appeal to possibility does not help us. Carnap has been seeking for a justification of the concepts upon which our logic is based, namely 'synonymy' and 'analyticity'. This he does by defining them in terms of systems of possible objects. Such systems, however, presuppose the very concepts he is trying to explain, since they are certainly "grounded on meanings independent of fact", which is what we mean by analyticity.

Carnap endeavours to clarify the difference between the extensional and intensional aspects of a word by comparing the entry (a) *Pferd*, horse, in the notebook of one of our hypothetical linguists studying the German language with the entry (b) *Pferd*, horse or unicorn, in the notebook of the other linguist. Carnap wishes to discover whether we have an empirical method for confirming the truth of either (a) or (b). He points out that as unicorns do not exist the two intensions ascribed to the word *Pferd*, though different, have the same extension—they refer to the same object. Hence, he argues, if the extensionalist theory were right there would be no way of empirically deciding between them.

To this we might reply that the above criticism only holds if the extensionalist's universe of discourse is restricted in the way Carnap restricts it, so as to include only spatio-temporal objects. It certainly would not apply if we took into account the world of our imagination. But even on a purist extensionalist approach one could always produce a picture of a unicorn as it appears, for example, in the British royal coat of arms. As a consequence of his circumscribed view of extension Carnap would have to regard even the picture of this mythical animal as being intensional in character. Nevertheless, the average citizen of the British Isles is usually more familiar with unicorns, as inhabiting

the world of mythology, than he is with some existing zoological specimens.

Carnap now outlines a method for deciding the truth of entries (a) and (b) in the linguists' notebooks. The linguist, in studying the German language (by observing the language behaviour of Karl), can begin his studies by making out the extensions of such predicates as *blau* and *hund*. With this simple vocabulary (acquired perhaps by pointing to the object, seeing whether Karl nods his head, etc.) he can go on to determine intensions by describing possible cases. He may, to quote Carnap, describe a unicorn (in German) as "a thing similar to a horse, but having one horn in the middle of the forehead". The linguist can then ask Karl whether he is willing to apply the word *Pferd* to it. A reply in the affirmative or negative will then confirm or disconfirm either (a) or (b).

Carnap believes that the extensionalist might perhaps object that the man-in-the-street is usually unwilling to say anything about non-existent objects. But as he himself points out (without drawing the obvious moral) the man-in-the-street is well able to answer questions about assumed or even non-existent situations. This is shown in ordinary conversations about alternative plans of action, about dreams, legends and fairy tales. However, the ability of the ordinary man to cope with alternatives largely arises from his acceptance of the world of imagination as an important part of his experience. The elements of the latter are for him not always sharply separated from those occurring in the external world. As we know only too well, myths play an important part in our culture.

Carnap's views on the nature of 'extension' bear some resemblance to those of Russell, when the latter argues that since one cannot name a person who is not there, the only way of dealing with mythological persons such as 'the present King of France' and *Romulus* is by the method of descriptions. Of this Gardiner has said, "To refute Russell's view that you cannot name a person who is not there, it is necessary only to quote *Romulus* as evidence that you can. And the thousands of fictional and mythological characters which we may remember will not improve upon that answer".<sup>8</sup>

<sup>8</sup> Sir Allan H. Gardiner, *The Theory of Proper Names* (Second Edition), Oxford, 1954, p. 66. The feature of descriptions which endear them so much to Russell is that they enable us to deal with the names of non-existing personages. Such names do not according to him refer to spatio-temporal objects, but simply imply that one and only one thing satisfies a certain condition.

## III

Carnap tells us that we can discover whether a given person has a particular linguistic disposition for the use of a specific predicate in two ways: (a) by producing the condition and seeing whether the response occurs (behavioural or operational analysis); (b) by describing the person's physiological state and with the aid of the appropriate laws calculating the linguistic responses he would make to any specified changes in his environment (structural analysis). But Carnap wisely admits that in the present condition of physiological knowledge (b) is not a very practicable method. He compares such a linguistic disposition with the ability of an automobile to accelerate on a horizontal road at a specific speed (*e.g.* ten miles per hour). The hypothesis that the automobile has this ability may, he argues, be tested either by driving the car and observing its performance, or by studying its internal physical structure and calculating the acceleration which would result under the specified conditions.

However, in the case of the automobile one knows exactly what an acceleration is. We know something about the internal structure of the motor and the mechanical laws it follows. Further, our calculations can be verified within a small degree of probable error. But a linguistic disposition, for example, to say 'cow' when one is presented, seems to be an entirely different matter. We have no guarantee that the response will occur when the stimulus is given. We deal with a complex physiological system of which we do not know whether a structural analysis (in terms of micro-physical laws) is even theoretically possible. Carnap's view implies something like a preconceived conception of the nervous system as a digital (or analogue) computer and would appear to be another version of the physicalism which one thought he had abandoned—namely that psychological statements could be translated into physical ones.

Carnap now comes to his *pièce de résistance* and proceeds to consider how we could investigate pragmatically the intensions assigned to its language by a robot. Unlike the case of a human being, it is assumed that we have a sufficiently detailed blue-print according to which the robot is constructed. Carnap endows this robot (which seems to be a 'knowledge by acquaintance and description' machine) with the abilities of organisation and the use of language, and gives it three input organs *A.B.C.* and an output organ. The function of *A* is to observe visually presented objects, *i.e.* recognise objects, that of *B* to receive a general description, *i.e.* a linguistic description of an object, and that of *C* to receive a predicate. The output organ can either affirm, deny

or abstain from responding, in accordance with whether or not the predicate applies to the presented object or description. The machine abstains from answering when it is, as it were, unable to 'make up its mind' whether or not the predicate applies, usually when the presented information is inadequate. Carnap does not tell us very much about the internal structure of the robot or what he means here by 'abilities of organisation and the use of language'. We can only surmise that it would cover at least learning and the ability to symbolise visual patterns by linguistic ones. He thus conjures up a robot of a rather advanced type, of which no models as yet exist. Without some plan of its underlying principles, we remain very much in the dark as far as its method of functioning is concerned.

Carnap now applies to the robot the method of deriving intensions by behavioural analysis. He assumes that we can begin to investigate its language extensionally by presenting objects at *A* and recording its responses. On the basis of such a preliminary survey we then present predicate expressions (descriptions) at *B*, using only these interpreted signs. In the light of the given responses we may determine what intensions the robot assigns to the words of its language. Or to put it differently, as when previously investigating Karl's linguistic behaviour, we begin by first pointing to objects, and after having arrived at a minimum vocabulary proceed to ask further questions in terms of descriptions formed from it. We test here, as it were, the machine's cognitive rather than perceptive behaviour, and in this way discover what meaning the sentences of its language have for it.

If on the other hand we want to use the method of 'structural analysis' to determine the 'intensions' of the robot, we need first, according to Carnap, to equip ourselves with a blue-print (describing the material state of the robot at time *t*). We can then, using the appropriate physical laws determining the robot's functioning, calculate the response it would make (Yes, No, ?) to various possible inputs in the form of visual objects, which could be given via *A*. If we begin with, say, the predicate *Q* (presented at *C*) and the observable properties *F*<sub>1</sub> and *F*<sub>2</sub>, we may find by calculation (much as we can calculate the acceleration of the automobile) that we will get "an affirmative response if and only if an object having the property *F*<sub>1</sub> is presented at *A*, and a negative response if and only if an object with *F*<sub>2</sub> is presented at *A*".<sup>9</sup> For Carnap this result indicates that the boundary of *Q*'s intension lies somewhere between the boundary

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<sup>9</sup> *Ibid.*, p. 44.

of  $F_1$  and  $F_2$ , which are presumably taken as incompatible in character. This seems to be a roundabout way of saying that the predicate  $Q$  refers to  $F_1$ , which determines its intension.

In this preliminary determination of intensions, the intensional boundaries between predicates may not always, we are informed, be precisely specified. There may be a zone of indeterminateness between them, *i.e.* a certain amount of vagueness and indetermination in their application to specific properties. Carnap recognises that for some predicates, for example those representing colours, the zone of indeterminateness may be quite small, *i.e.* the number of '?' responses will be low. In this case our preliminary determination of the intensions of the robot's language will be fairly precise.

Carnap gives one the impression here that such 'intensional vagueness' is somehow due to our inability to grasp the precise significance of the predicates obtained in this sample of the robot's language. However, this would only hold in the case where the robot's language was a completely strange one to us. But it would not apply if the language was one we already understood, *e.g.* English. In this case, vagueness in the machine's application of predicates to specific properties would have to be traced to a deficiency in its own powers of visual analysis<sup>10</sup> (which in a human being we might term inadequate concept formation). For Carnap the boundaries between properties would appear to be clear-cut, and the properties themselves discrete and objective. If the machine was fitted with a more adequate visual analyser (or perhaps learning organ) it would manifest a greater *finesse* in its application of predicates to properties.

Having obtained a sample of the robot's vocabulary (by calculations concerning input  $A$ ), the investigator can, according to Carnap, proceed and make calculations concerning descriptions (constructed out of this vocabulary) which could be presented at  $B$ . The following result, we are told, may be obtained for a predicate  $P$  and properties  $G_1$  and  $G_2$ . If we present the predicate  $P$  at  $C$  and any description  $D$  constructed from this sample vocabulary at  $B$ , the robot will give "an affirmative response if and only if  $D$  (as interpreted by the preliminary results) logically implies  $G_1$ , and a negative response if and only if  $D$  logically implies  $G_2$ ".<sup>11</sup> This apparently means that the intension of  $P$  is determined by the description  $D$  which is, as it were, an inflated description of  $G_1$  which it may be taken as symbolising. In this way, Carnap argues, we can calculate the robot's responses

<sup>10</sup> And this would certainly have to show itself in the form of a certain imprecision in the calculations we make.

<sup>11</sup> *Ibid.*, p. 44.

to specific descriptions, and thereby obtain a more comprehensive account of its language and the meaning assigned to it.

Since the machine, like the human being, manifests 'intensional vagueness'—a certain imprecision in the drawing of boundaries between the intensions of predicates—this would show itself in a corresponding imprecision in a certain sub-class of our calculations. And to this sub-class we would have to assign the value '?'.

Returning to the actual functioning of the machine we then see that it employs a rigid trichotomy in its responses (Yes, No, ?) rather like that of a three-valued logical system. It replies 'Yes' when it can distinguish a property corresponding to the applied predicate (or description), 'No' when the predicate is incompatible with the distinguished property, and '?' when it is unable to decide where to draw the boundary of the predicate's application. But what clearly emerges is that in all these cases both predicate and descriptions refer to some sort of entity or property which gives them their intension.

It should be pointed out, however, that in the case of human beings, instead of this trichotomy we would get something more like a continuum of responses, as is the case, for example, when we grade or rank extensive magnitudes. Further, there will be a certain amount of arbitrariness as to where we draw the line between properties, as they usually blur into each other. Our boundary decisions will also tend to vary in accordance with the contextual situation in which we find ourselves. As Strawson puts it when speaking of predicates, "The boundaries are more like areas of indeterminateness than frontier-lines".<sup>12</sup> Carnap, on the other hand, conceives properties as being independent and discrete. He therefore resolves 'intensional vagueness' into something like 'subjective error', which it is assumed prevents us as well as the robot from grasping the precise boundaries of properties as they exist in nature (or as possibilities).

According to Carnap, we can also discover what sort of reply the machine would give to descriptions referring to non-existent objects and causally impossible ones. The robot could be presented with descriptions of unicorns, minotaurs and other mythical beasts. We might, for example, ask whether we can apply the predicate 'man' to a minotaur and obtain a '?' response, since an appropriate decision procedure for resolving this classificatory problem may not have been built in. It might even be confronted with a blueprint of itself, and asked to decide whether it is a 'thinking being'. By giving it a large number of such border-line concepts, we might involve the machine in grave difficulties.

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<sup>12</sup> P. F. Strawson, *Introduction to Logical Theory* (1952), p. 5.

## IV

In some ways this mechanical approach seems to be a reversion to Carnap's earlier position,<sup>13</sup> when he asserted that the meaning of a word can be specified by means of formal rules, either by (a) translation, or (b) definition. An example of (a) would be "cheval = horse", and of (b) "elephant = animal possessing such and such pachydermous characteristics". However, in both cases we fall back on synonymy, which is certainly not a formal rule (as far as ordinary language is concerned). This can only be the case in an artificial semantic system in which it is arbitrarily defined as one. The 'logical implication of a property by a description', as seen e.g. in Carnap's account of his method for computing the intensions of a robot, is simply a formal translation of this kind. The difference seems to be that here the synonymy relation is built into the machine (whose operations can be determined by calculation) instead of occurring as a feature of a linguistic statement. There is thus no radical difference from his earlier position that the meaning of a word can be specified by formal rules. The blue-print of the robot does not then seem to differ essentially from that of an abstract semantic system. The only difference is that the postulates and definitions are now built into the machine.

Carnap's belief, that the concept of intension can be applied even to a robot, does imply that the understanding of the sense of a word is not an essential element in its meaning for him. This no doubt is due to Carnap's assumption that the relation between a predicate and a property (which determines the predicate's intension) is an objective one. But even if we accept the view that concepts have an objective character, can we talk of a statement as having an "intension", unless it is used by someone who understands its significance? We are not merely concerned with the correct application of a predicate to a specific property (which could occur by chance), but also with our understanding of the significance of the predicate. This is one of the reasons why some philosophers, such as G. E. Moore, have insisted on the difference between "uttering" and "asserting" a statement, where "asserting" implies understanding. Though a robot might utter the statement "London is large", it would not be asserting it in this sense. However well-organised its vocabulary might be, we should not usually say that it understood the significance of the words it was uttering.

Further, in recent years the sharp distinction between names and descriptions (the extension-intension dogma), on which

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<sup>13</sup> Cf. R. Carnap, *Unity of Science*.

Carnap's hypothetical robot is based, has fallen into some disrepute. This distinction, it is pointed out, arises from the assumption that the meaning of an expression is identical with the object to which it refers or applies. As descriptions of mythical objects such as Pegasus, which do not refer to existing objects, are nevertheless meaningful, it is assumed that they must be about something. But, since it is not wished to make their meaning depend on anything psychological, they are taken as predicates referring to logical entities, which are symbolised by existential quantifiers. Nevertheless, as Strawson, for example, points out, this requirement, that for a predicate to have a meaning it must refer to a specific individual, is not fulfilled even with proper names. The same name can be applied to many different persons and things. Further, for a name or a description to be significant, it is not necessary that the object named or described should exist. It is sufficient if one can describe or imagine circumstances in which its use would make it true or false.<sup>14</sup>

As Gardiner puts it, "for Language it is a matter of complete indifference whether the thing named or described has or once had an external existence, or rather . . . whether or not it has ever come before its namer or describer".<sup>15</sup> Gardiner is of the opinion that "the function of Language is purely instrumental, and that, provided words can be found to make a listener think of something to which the speaker wishes to make reference, it matters not whether a proper name is used or a description comprising several words. The words are mere scaffolding to be removed when its purpose is fulfilled".<sup>16</sup> His point then is that in communication we are not primarily concerned with the logical structure and the symbolism of the language we use, but with its psychological effectiveness in bringing about a certain result.

Once again Carnap seems to base his position on the view that logical and mathematical systems are good models for ordinary language. He seems to be arguing by analogy that just as mathematical symbols are taken as standing for precise concepts, so the words of a language (categorematic and syncategorematic) have a similar referential character, though they refer not merely to concepts but also to things. Carnap thereby overlooks the difference between a language in use, which may not have a precise referential function, and a mathematical system, which on the logistic view usually has.

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<sup>14</sup> Cf. Strawson, *ibid.*, p. 185.

<sup>15</sup> *Ibid.*, p. 66.

<sup>16</sup> *Ibid.*, p. 61.

## SOME FUNDAMENTAL PROBLEMS OF INDIRECT MEASUREMENT

By BRIAN ELLIS

In a paper published recently in this journal,<sup>1</sup> I made a distinction between direct and indirect measurement, and discussed certain problems of direct measurement. It was argued that most, if not all, of the important directly measurable quantities are measured by logically similar procedures. Such quantities were described as fundamentally measurable quantities, and the procedures as fundamental measuring procedures. But since few quantities are measurable by the same procedures over the whole of their range, this terminology was perhaps somewhat misleading. Ordinary distances may be fundamentally measurable, but subatomic and interstellar distances are not. The distinction I wished to make was not so much a distinction between different sorts of quantities, as a distinction between different forms of measurement—direct and indirect measurement.

I now wish to discuss certain problems which arise in connection with indirect measurement, i.e. measurement which depends upon prior measurement. But first it should be noted that measurement may depend upon prior measurement in different ways. One way in which it may do so is illustrated by temperature measurement. The measurement of this quantity always depends upon the measurement of some other quantity which in special circumstances is taken as a quantitative criterion for temperature. Likewise, the measurement of pressure always depends upon the measurement of something else. Where such is the case, where the measurement of a quantity  $p$  always depends upon the measurement of some other quantity or quantities,  $p$  will be described as a “derived magnitude” and the measurement of  $p$  will be said to be a form of “derived measurement”.

A second way in which measurement may depend upon prior measurement is illustrated by the measurement of very large distances, masses and time intervals. Such measurement, like derived measurement, depends upon the adoption of a quantitative criterion for the quantity  $p$  to be measured, but it also depends, to some extent, upon the direct and fundamental measurement of  $p$  in a more restricted range. Measurement of this kind, which seems to straddle fundamental and derived measurement, will here be described as “mixed measurement”.

I do not know in what other ways measurement may depend

upon prior measurement. But in this paper I propose to discuss some problems which arise in connection with these two forms of indirect measurement.

### I. Derived Measurement

I will limit my discussion of derived measurement to temperature measurement, although I think that most of what I have to say about temperature measurement is applicable to other forms of derived measurement.

First, it should be noted that the problems involved in setting up a scale of temperature are somewhat different from those involved in setting up a scale of, say, length. Temperature is not measurable directly. To establish a scale of temperature which does not suffer from the arbitrariness of Mohs' hardness scale, some measurable property which varies with temperature is needed. Such a property will be called a thermometric property. Length, volume, pressure, electrical resistance, and solubility are examples. But to establish a scale of length, a measurable property varying with length is not required. Length is thus directly measurable. A scale of length could, of course, be set up indirectly. Weight, for example, could serve as a criterion for length, and we could define the unit of length as that of a piece of, say, platinum wire manufactured in such and such a machine and having a weight of 1 gram. But no such quantitative criterion is necessary (or desirable) for the establishment of a scale of length. Whereas a criterion does appear to be necessary for temperatures.

This being the case, there are two problems involved in setting up a temperature scale which do not occur in setting up a scale of length: viz., which thermometric property we are to choose, and what functional relationship we are to suppose to hold between the measure of this property and temperature. The first is the problem of choosing a property to serve as a criterion for temperature, and the second is the problem of deciding how the thermometric property is to be related to temperature. Following Mach,<sup>2</sup> we will call these problems:

- (1) the problem of choice of thermometric property, and
- (2) the problem of choice of principle of correlation.<sup>3</sup>

A second point is that, while length is an additive property, temperature is not. There are physical counterparts of the arith-

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<sup>2</sup> E. Mach, *Principien der Waermelehre*, Chapter 3.

<sup>3</sup> This problem of choice of principle of correlation is, of course, somewhat different from the problem we noted in connection with fundamental measurement.

metrical operations of adding or subtracting length, but none for temperatures. Hence, in setting up a scale of temperature, there is no need to adjust it so that any operations with objects at temperatures  $t_1$  and  $t_2$  will produce an object whose temperature is  $t_1 + t_2$  or  $t_1 - t_2$ . Nor, for that matter, need we adjust it so that any other mathematical operations on temperature numbers should have a physical interpretation. Temperature is not an additive property: it is not a 'multiplicative' one either. We are thus faced with quite a different set of problems in setting up a scale of temperatures. And since one of the objects of this paper is to discuss the significance of the numbers assigned to objects as a result of measuring operations, I now propose to consider what reasons there may be for choosing one thermometric property rather than another, or one principle of correlation rather than another.

#### *The choice of thermometric property*

Objects can be arranged in a crude order of temperature without instrumental aids. The basic ordering relationships are 'feels hotter than', 'feels as hot as' and 'feels colder than'. But the order given by these relationships is extended and refined by our knowledge of what makes a body hotter or colder, and of what effects heating or cooling has on bodies. Thus we can say: "Today must have been colder than yesterday—there was ice on the ground", or "The temperature of melting lead is less than the temperature of melting aluminium, because lead melts before aluminium".

But objects can be arranged in more or less the same order by any of a large number of groups of ordering relationships involving thermometric properties. The simplest class of such ordering relationships refers to some object T having some thermometric property p which varies with the temperature of objects A, B, C . . . when T is placed in some specifiable relationship to them. The objects A, B, C . . . may then be arranged in the order of p. If this order is more or less the same as the order given by heat sensation, then p of T may be described as a criterion for temperature. If p is a measurable property, then the measure or reading of p may be used to provide us with a measure of temperature, and the object T may be described as a thermometer.

The problem we are here concerned with is the problem of choice of p and of T. What reasons can we give for choosing one property p rather than another, or one object T having the property p rather than some other object S which also has this property? We feel, I think, that any measure of temperature which is thermometer-dependent, that is, depends on the particular

properties of some particular object, cannot, save accidentally, be of much significance. Even if it depends on the special properties of some particular substance or group of substances, we feel this. What we feel is needed is a universal property of matter as a thermometric property, and a thermometer-independent procedure for determining the temperature of an object. At any rate, I now propose to consider whether it is possible to find such a property or such a procedure.

Two empirical facts should first be noted. The first is the existence of reproducible heat states, that is, states of bodies which can be described without involving any ideas of temperature, and which always give the same reading on *any* thermometer no matter what thermometric property or substance it employs. An example of such a state is water at the boiling point. The second empirical fact is sometimes referred to as the zero'th Law of Thermodynamics. It is the fact that after sufficiently long contact away from heat sources or refrigerators, the thermometric properties of bodies cease to vary, and their equilibrium is not disturbed by subsequently switching them around. In other words, if A is in thermal equilibrium with B and B with C, then A will be in thermal equilibrium with C.

These two facts together provide us with thermometer-independent criteria of temperature equality. That is, because of these two facts we do not need to specify a thermometer in order to be able to say of two objects that they are *at the same temperature*, or of one object that its *temperature has remained the same*. Temperature equality, whether of co-existent or non-co-existent objects, is not thermometer-relative. We can say of two objects that they are equal in temperature and mean by this that they are in thermal equilibrium with each other. And we say that the temperature of any given object has remained the same, and mean by this that its thermometric properties have remained constant.

These two points can best be illustrated by considering the situation which would arise if the zero'th law were not true, or if there were no reproducible heat states. Consider first the situation that would arise if thermal equilibrium were not a transitive relationship. Suppose that three objects A, B and C are placed together thus: | A | B | C |, and that under these conditions their thermometric properties do not vary. But suppose that on changing A and B round the equilibrium is disturbed. Now if B is chosen as a thermometer, we will have to say that A, B and C are all at the same temperature. Whereas if C is chosen we will have to say that at least A and B are at different temperatures. Therefore temperature equality under these cir-

cumstances must be thermometer-relative. And it would only make sense to speak of two objects being at the same temperature if the thermometer with respect to which they are said to be equal were specified.

The second point is that if there were no fixed and reproducible heat states, there would be no background of order against which the temperature of an object could be said to have changed or not to have changed. There might, indeed, be movements or changes within the temperature order, but only of one object with respect to another.

There are, then, thermometer-independent criteria for temperature equality, and thermometer-independent descriptions of fixed heat states. But this does not mean that there are thermometer-independent procedures for assigning temperature numbers to objects. We can, of course, assign numbers to the fixed heat states to represent their relative position in the temperature order, 100 to boiling water, say, 0 to water at the freezing point, 50 to boiling alcohol and so on. But this is quite arbitrary. 212 would, for this purpose, do as well as 100, 75 as well as 50, and 10 as well as 0.

Now how is this arbitrariness to be avoided? I have suggested that the appearance of arbitrariness could be avoided if a thermometric property which is a universal property of matter could be chosen as a criterion. That is, if there were a thermometric property possessed by all substances which always had the same value in the same fixed heat state, then the measure of this property, or some function of it, could be taken as the measure of the temperature. And this would be the least arbitrary choice that we could make. Now in fact there is such a property. The efficiency of a perfectly reversible heat engine working between a heat source and heat sink which are in fixed heat states is independent of the nature of the working substance. And the thermodynamic scale which is based upon it is the least arbitrary that we have.

To show this, let us compare it with the centigrade mercury scale. Roughly speaking, the centigrade mercury scale is set up in the following way. A vacuum tube containing mercury is immersed in boiling water. When equilibrium has been established, a mark is made at the mercury surface and assigned the number 100. It is then immersed in freezing water, and when equilibrium has been established the new level is marked "0". The glass is then calibrated according to length (or, more accurately, volume). At a mark half-way between "0" and "100", the mark "50" is placed—and so on.

Now about this calibration we can ask the following questions:

1. Why choose mercury—why not alcohol? If you do choose alcohol, you will be led to assign different numbers to the same objects under the same conditions.
2. Why choose *volume* of mercury as a criterion for temperature? If you choose, say, electrical resistance, you will get another scale.

A satisfactory answer cannot be given to either of these questions. But in the case of the thermodynamic scale, neither of these questions arises. The first cannot be asked, since, no matter which substance is used as the working substance in a reversible heat engine, the scale will be the same. And the second does not arise since no other thermometric property which is a universal property of matter and which always has the same value at the same fixed point has yet been discovered.

So, to this extent, the thermodynamic scale is less arbitrary than the mercury scale, and for similar reasons I believe that it is less arbitrary than any other scale that has yet been proposed.

But this does not mean that the thermodynamic scale is not arbitrary in any way. There does, I think, remain an essential element of arbitrariness in the choice of *any* temperature scale based on indirect measurement. I do not mean, of course, that it is arbitrary in the choice of unit. All scales are arbitrary in this respect—direct as well as indirect ones. Even in the measurement of length by metres we arbitrarily assign the number 1 to an object kept in Paris. The number 10 would do just as well. What I mean is that there is a choice of functional relationship between the measure of our universal thermometric property and temperature. Shall it be linear, or logarithmic, or exponential, or what sort of relationship? This choice is what Mach called the choice of principle of correlation. And to this we must now turn.

#### *The choice of principle of correlation*

As the argument has been presented so far, it would appear that good reasons can be given for the acceptance of the thermodynamic scale of temperature rather than any other without its being necessary even to mention theories about the nature of heat and temperature. And this may be taken to suggest that such theoretical considerations have no rôle in the choice of temperature scale. But this suggestion would certainly be false. Theoretical considerations (as well as practical ones) led Black to adopt the mercury scale. Theoretical considerations were behind Dalton's rejection of it. And, finally, theoretical considerations gave strong support to the adoption of the gas scale. Historically, it is quite

false to suggest that theories about heat and temperature have played no part in the choice of temperature scale.

But though this may be so, we may still ask whether the acceptance of any theory about heat and temperature, together with the facts upon which it is based, necessarily involves the acceptance of a particular scale or type of scale. Reasons, even good reasons, need not be logically conclusive. We may ask whether theories of heat and temperature even give us any good reasons for adopting a particular kind of temperature scale. For the fact that they have been thought to do so does not imply that they do. It will be argued here that, while theoretical considerations may have some bearing on the choice of temperature scale, there yet remains an essential arbitrariness on which theories of heat and temperature have very little bearing. This arbitrariness lies in the choice of principle of correlation.

The principle of correlation, as it is here understood, is the functional relationship between the temperature number assigned to an object and the measure of its chosen thermometric property, i.e. the function  $f$  such that  $\theta^\circ = f(p)$ . The only necessary restrictions which must be placed upon  $f$  are that  $\theta$  must be defined for every value which  $p$  may in fact assume, and the numerical order of the temperature numbers assigned to objects according to this principle must correspond to the temperature order. If these conditions are not fulfilled we would not be justified in calling the number  $\theta$  assigned to an object a measure of its temperature.

The second of these points I take to be sufficiently clear. The first may be illustrated by the following example. Suppose we were to adopt the principle of correlation  $\theta = \log(1 - l_{\text{ice}})$  where  $l$  is the length of the mercury column in an ordinary mercury thermometer,  $l_{\text{ice}}$  the length at the ice-point,  $\theta$  is then defined in the field of real numbers for values of  $1 > l_{\text{ice}}$ . But there would, of course, be conditions under which  $1 < l_{\text{ice}}$ , and  $1 = l_{\text{ice}}$ . According to this principle, then, when  $1 = l_{\text{ice}}$ ,  $\theta = -\infty^\circ$ . But what are we to say is the temperature of a certain mixture of ice and salt which gives a reading of  $1 < l_{\text{ice}}$ ? It must have a temperature, since it can be placed in a unique position in the temperature order. It would seem, then, that if we were to adopt this as a principle of correlation we would have to say that its temperature is less than  $-\infty^\circ$ . The only alternative is to reject the principle of correlation. And this course is certainly the one that would be taken.

This points to what I think is a necessary condition for describing a procedure for assigning numbers to objects as a procedure for measuring some quantitative property. It is that the

number sequence should not be exhausted either above or below before the sequence of objects arranged in the order of this property.

Granting these restrictions, there are nevertheless infinitely many possible principles of correlation between temperature and thermometric property. Historically most but not all of these have been linear in form. Amontons used a linear relationship of the form  $\theta \propto p$  where  $p$  is the pressure of air held at constant volume. Celsius and Fahrenheit calibrated their scales by taking equal increments in the volume of a sample of mercury to correspond to equal temperature intervals. But linearity was rejected by Dalton who took equal increments in volume to correspond to equal increments in heat capacity. He suggested that the relationship between volume and temperature was a logarithmic one.

The initial reasons for adopting a linear correlation principle were no doubt reasons of simplicity. Why assume a complicated relationship when a simpler one will do?

But Black at any rate considered that his calorimetric experiments confirmed the assumption of linearity, and Dalton, considering similar experimental results, concluded that the assumption of linearity must be false. It is clear that both Black and Dalton believed that good reasons other than reasons of simplicity could be given for choosing one principle of correlation rather than another. If they were right—if good reasons could be given for choosing one principle of correlation rather than another—then the whole problem of choice of thermometric property could perhaps be circumvented. For it would matter very little which thermometric property or substance were chosen in a standard thermometer, if this thermometer could be calibrated on the basis of, say, calorimetric mixing experiments.

But there is a fundamental weakness of all *theoretical* arguments for adopting one principle of correlation rather than another. It is, that the very difficulty which these arguments seek to overcome crops up again in the theory. The problem is the problem of choice of principle of correlation, *i.e.* of a functional relationship between thermometric property and temperature. A theory of temperature proceeds by saying what temperature is. On the Caloric Theory, temperature is a measure of the pressure of caloric. On the Kinetic Theory, temperature is a measure of the average kinetic energy of the molecules. But why should we make such assumptions? Only because they enable us to predict the experimental results or laws obtained by use of uniformly, that is, linearly calibrated instruments. If, for example, a logarithmically calibrated instrument had been used from the start, *i.e.* if  $\theta \propto \log V$  had been used as a principle of correlation in the 18th century,

then the empirically discovered gas law of Gay-Lussac would have been of the form  $PV = R\theta^o$ . To account for this law, it would then have been necessary to assume in the Kinetic Theory that temperature is the logarithm of the average kinetic energy of the molecules. And this could then have been taken to support the original logarithmic correlation principle.

The only argument which I can see against the adoption of a non-linear correlation principle is not a theoretical argument at all, but an argument from simplicity. It could be argued with some justification that  $PV = R\theta$  is a much simpler mathematical form for the gas law than  $PV = R\theta^o$ , and that the results of calorimetric mixing experiments would be much easier to predict on the assumption of linearity than otherwise. But, although this may be so, there is yet no guarantee that a linear scale is the simplest possible temperature scale. Indeed, it seems to me that from the point of view of the mathematical simplicity of temperature laws, the substitution of  $\frac{1}{\theta}$  for  $\theta$  (the absolute temperature) would achieve some further simplification. It would mean that instead of the gas law,  $PV = R\theta$ , we should have:  $PV\theta = \text{const}$ , and instead of the Second Law of Thermodynamics

as it applies to thermodynamically reversible processes,  $\Sigma \frac{Q}{\theta} = 0$ ,

we should have:  $\Sigma Q\theta = 0$ . At any rate, it is by no means evident that temperature laws assume their simplest mathematical form when a linear correlation principle is adopted. If they do, then it is only an historical accident. But whether or not they do is not a question which theory can decide.

Thus, whatever theoretical considerations we may take, there remains an essential arbitrariness in the choice of principle of correlation. Only reasons of simplicity can guide us in this choice.

But surely, it will be said, theories do determine at least some aspects of our temperature scale. Is there not a natural zero for temperature? And is this not a theoretical prediction? Yes, this is certainly true. But let us see what it means and what it does not mean. It does not mean that there is a lowest member of the sequence of possible heat states. Indeed, the Third Law of Thermodynamics suggests that this is not the case. It does not mean that there is, or might be, a heat state with which the association of the number zero is particularly appropriate in view of the special mathematical properties of zero. The proposition  $n \pm 0 = n$  cannot be used to predict the result of any physical operation involving objects at temperatures  $n^\circ A$  and

$0^{\circ}\text{A}$ . To say that there is a natural zero for temperature, therefore, cannot be quite like saying that there is a natural zero for, say, distance. What it does mean, however, is that the temperature in  $^{\circ}\text{C}$  always appears in fundamental temperature laws together with a constant (+273), and that a simplification of the form of these laws can therefore be achieved by a change of variable, *i.e.*, a change to the absolute scale. It also means that on the absolute scale there is a lower limit to the temperature numbers which are significant, and this conveniently is the lower limit of the real positive numbers, *viz.* zero.

To suppose that there is an object whose temperature is less than  $0^{\circ}\text{A}$  is to make a supposition which can be true only if certain accepted temperature laws and theories are false. For instance, to substitute a negative number for  $t_0$ , the temperature of the sink (condenser) in a reversible heat engine, is to give a value for the efficiency of this engine greater than 100%. Moreover, a negative temperature number on the absolute scale would have no interpretation on the kinetic theory of temperature.

Theoretical considerations, then, do influence us in the determination of certain features of our temperature scale. For if on any arbitrarily chosen temperature scale there are theoretical limits to the temperature numbers which are significant, we prefer to change the principle of correlation so that these limits are reflected by the properties of the number system.

But even when this is done we must, as Mach has pointed out,<sup>4</sup> always be careful about drawing inferences from the properties of the number system. From the fact that the set of real positive numbers is everywhere dense it does not follow that the sequence of heat states is everywhere dense. From the fact that this set of numbers is bounded below, but unbounded above, it does not follow that the set of heat states is bounded below and unbounded above. This is an important point, and we need to be constantly reminded of it, not only in connection with temperature measurement, but in connection with other kinds of measurement as well. For consider the effect on our thinking of a change to a non-linear correlation principle, where the numbers assigned to objects according to this principle are related to absolute temperatures by a relationship of the form  $T = k \log A$ , where  $A$  is the absolute temperature, and  $T$  is the new temperature. Then  $0^{\circ}\text{A}$  would be represented on this new scale by  $-\infty^{\circ}\text{T}$ . Now it seems to me that if we had been brought up to use such a scale, the Third Law of Thermodynamics would have been taken as self-evident. Of course, we cannot reach  $-\infty^{\circ}\text{T}$ . Moreover, the

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<sup>4</sup> E. Mach, *Principien der Waermelehre*, Chapter 3.

question of whether there could be an object whose temperature is  $<-\infty^{\circ}\text{T}$  would have been rejected as absurd. But, obviously, there is nothing self-evident about the Third Law, and it is not absurd to ask whether there could be an object which is colder than one at  $0^{\circ}\text{A}$ .

The psychological effect of adopting one principle of correlation rather than another is clearly illustrated by this example. And although, from a mathematical point of view, a change of principle of correlation amounts simply to a change of variable, which may or may not simplify the mathematical form of temperature laws, the whole effect of such a change is likely to be much greater than this. It is likely to change quite radically our thinking about the subject of temperature. And this is so because we easily and naturally think that the properties of the sequence of heat states is reflected in the properties of the numbers by which we designate them.

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In conclusion, it seems to me that the chief reasons for adopting one principle of correlation rather than another for a derived magnitude are reasons of simplicity—simplicity in the mathematical form of laws involving this quantity. And although this problem of choice of principle of correlation is somewhat different from the corresponding problem which occurs in fundamental measurement, it seems to me likely that similar considerations would apply. If our physics could be simplified by a change of length scale or mass scale, then these changes should be made even if it means abandoning additivity.

## II. *Mixed Measurement*

No magnitude is directly and fundamentally measurable over the whole of its range. Inter-atomic distances are not fundamentally measurable. Nor are intergalactic distances. Sub-atomic masses are not directly measurable. Nor are the masses of the stars or planets. To measure such distances or masses we need to adopt a quantitative criterion, and a principle of correlation. It may, therefore, be wondered whether in these extreme ranges length and mass should be regarded as derived magnitudes, much like temperature. And, if so, whether the same warnings should apply.

There are, I think, certain differences between the logical problems of measurement in extreme ranges and those of derived measurement. But it is my view that these differences are only superficial, and that the measurement of very large or very small

distances or masses is logically very like the measurement of, say, temperature. If I am right in this, then it seems to me that the question of whether space is infinite in extent is logically very like the question of whether there is an upper or lower limit to temperature. It is ambiguous in the same sort of way, and it contains similar traps.

It is not, however, my intention to discuss specifically distance or mass measurement in extreme ranges. I propose instead to consider one kind of time measurement as an example of mixed measurement, and to apply my analysis to the problem of whether the universe does or does not have a beginning in time. But what I have to say will, I believe, be of general significance. Similar things could be said about whether space is infinitely subdivisible, or whether it is infinite in extent, or whether there are "atoms" of time interval.

By "mixed measurement" I understand a form of measurement which, as it were, straddles fundamental and derived measurement. Such measurement always depends upon the adoption and measurement of a quantitative criterion for the quantity  $p$  to be measured. But it also depends to some extent upon the direct and fundamental measurement of the quantity  $p$ . Just how this is so can best be seen by considering an example. And the following considerations concerning time measurement are meant to illustrate the features and to bring out the problems of that form of measurement which I have described as "mixed measurement".

### *The measurement of time*

Time, like temperature, is a measurable quantity. Events can be arranged in a unique order of time (at any rate by a set of relatively stationary observers). We must enquire then what type of measurable quantity it is. Is it directly or indirectly measurable? There seems to me to be no clear answer to this question. It is true that if we are timing an event—a foot-race for example—we can, by starting a pendulum swinging as the race begins, count the number of repetitions in this event (*i.e.* the number of pendulum swings) necessary to produce equality in respect of time interval with the given event. We may judge, for example, that the time for the race was equal to the time for ten pendulum swings. And this suggests that time interval should be regarded as a directly and fundamentally measurable quantity—like length.

But there is a difficulty in this. For, once the race is over, we cannot 'go back' and time it in this way. Only present, not past events, can be timed directly. So it looks as if we ought to

say that while present time intervals are directly measurable, past time intervals are not. And, odd as it may sound to say this, I think that this is what we must say.

I will, therefore, distinguish between what I shall call 'dating' and 'timing' procedures. By a timing procedure, I will mean what is ordinarily meant by this term. And, according to ordinary usage, it makes no sense to speak of timing now a past event. I cannot intelligibly say that I am now timing the 100 metres final in the last Olympic Games: By the term 'dating procedure', however, I wish to include any procedure for *measuring* the time interval between two past events or between a past and a present event. This is a special use of these words, and it is not connected very closely with their ordinary usage.

First we should notice that most of the dating done by historians cannot properly speaking be described as measurement. In most matters, historians have to rely on the reports of witnesses, or upon reports of these reports. They use techniques of sifting them for consistency, of checking them off against one another, of building up a coherent picture of the course of events, and so on. But none of this is measurement. I cannot be said to have measured the length of a table if all I have done is to consider the reports of three people who claim to have measured the table, no matter what investigations I may make concerning the character or the circumstances of the individuals concerned. It does not matter how well founded my judgment as to the length of the table may be as a result of these considerations: I cannot be said to have measured its length unless I have myself carried out the appropriate measuring procedure. Similarly, I wish to argue, a historian who relies upon diaries, reports, reports of reports and so on in assigning a date to an occurrence, cannot be said to be measuring anything.

However, there is something which I wish to describe as the measurement of past or past-present time intervals. The geologist who takes the salinity of the sea as a criterion for the age of the solid earth may, by measuring salinity, measure the age of the solid earth. If, in a less simple way, he takes the quantities of radio-active substances in the earth's crust as a criterion for the age of the earth he is measuring the age of the earth by an alternative means. Similarly, the archaeologist may measure the age of something by employing radio-active carbon criteria. The cosmologist may measure what he calls "the age of the universe" by measuring the red shift of distant nebulae. A man may measure the age of a tree by counting the number of annular rings. And so examples may be multiplied.

In each case, something is accepted as a quantitative criterion

for the age of something—that is, for the length of a past-present time interval. In each case, a principle of correlation between this quantitative criterion and age must be adopted. In each case, age is determined by measuring this quantitative property and employing the adopted principle of correlation.

The measurement of age, therefore, seems to involve similar problems to the measurement of temperature. There is a problem of choice of temporal property, and a problem of choice of principle of correlation. And it seems that age, like temperature, must be regarded as an indirectly measurable quantity.

But there is at least one very striking difference. This difference lies in the existence of a sort of correspondence principle. That is, there must be a correspondence of the results of timing and dating. Otherwise we should say that one or the other is inaccurate. Suppose, for example, that a certain process has been timed. Then if, afterwards, the beginning of this process can be dated by a dating procedure, then the results of these two investigations must agree. If they do not agree, then we would say that a mistake must have been made—or, eliminating this, that the dating procedure or principle of correlation employed is inappropriate.

And so it may be argued that really neither of the above problems exists. For whichever quantitative property we choose as a criterion for age, an individual principle of correlation can be determined experimentally by using this 'correspondence principle'. That is, we can time the changes in this property, and by this means determine how the measure of this property varies with age. And further, it may be argued, this is just what we do. We *time* the changes in radio-activity of a given substance, extrapolate the curve, and obtain *empirically* a principle of correlation between degree of radio-activity and age of sample. There is nothing arbitrary about this principle of correlation. It is fixed and determinate. And hence, in age measurement, there is no problem of choice of principle of correlation.

This argument is, I believe, a sound one. Principles of correlation between temporal property and time interval can be established by direct timing procedures—but only over very limited time ranges. A principle of correlation is a mathematical conversion formula. In this case it must be of the form

$$A = f(p)$$

where  $A$  is the age of a given event-type,  $p$  is the measure of a temporal property, and  $f$  is a monotonic function. But the validity of this formula can only be established for very small values of  $A$ —at most a few thousand years. In the dating of very distant past events we must extrapolate this relationship; and

there arises in place of what I have called the problem of choice of principle of correlation a new problem which, for convenience, I shall call the problem of choice of principle of extrapolation. For all that is required of the extrapolated principle of correlation is that, for very small values of  $A$ , it merges into the principle

$$A = f(p).$$

Thus any principle of correlation

$$A = \phi(p)$$

which is such that, as  $A \rightarrow 0$ ,  $\phi \rightarrow f$  will do as an extrapolated principle of correlation.

Consider an analogy. Suppose that temperature were a directly measurable quantity between  $0^\circ$  and  $0.1^\circ\text{C}$ . It would then be the case that over this range of temperature any principle of correlation between a thermometric property and temperature could be checked by direct measurement. But it seems to me that it would be an utterly fantastic assumption to suppose that because a given principle of correlation can be shown to hold in this temperature range it would hold over the whole temperature range. It would indeed place some restrictions on the choice of extrapolated principle of correlation. But there would nevertheless remain a very wide area of choice.

Thus I maintain that in the discussion of such questions as the age of the earth, of the solar system, or of the universe, age must here be regarded as an indirectly measurable quantity—more like temperature than like length. Dating is not, of course, exactly like temperature measurement. The existence of the “correspondence” principle between timing and dating does make dating logically somewhat different from temperature measurement. But, at the same time, it is logically very different from fundamental measurement, and it seems to me to be less misleading to consider dating on analogy with derived measurement.

#### *The age of the universe*

To conclude this paper, I would like to make some remarks concerning the question of the age of the universe.

When we say “The temperature of this object is  $400^\circ\text{C}$ ” just what do we mean? We mean that by following any of a number of well-specified temperature measuring procedures with sufficient care, anyone would be led to assign the number 400 to the object in question. When we say that there is a lower temperature limit at  $-273^\circ\text{C}$ , what we mean is that, no matter which object is taken under whatever circumstances, we will never be led to assign a number to it  $< -273$  provided that we follow a recognised temperature measuring procedure with sufficient care. It does not

matter for the moment what reasons we may have for saying such a thing, this is what we ordinarily mean.

There is, however, a second sense in which we may speak of a lower temperature limit. In this sense there exists a lower temperature limit if, and only if, an object can be found or produced which is such that no colder object can ever be found or produced. Mach, for example, thinks of the possibility of a lower temperature limit like this. He says: "Whether the sequence of heat states is bounded above or below can only be decided by experience. If one cannot find a body which is warmer or colder than a body of a certain temperature, only then is there a limit."<sup>5</sup> A little reflection will soon show us that these two senses are quite distinct from one another. There is no contradiction in saying that we can always produce an object which is colder than a given object, and at the same time that no matter which object is taken under whatever circumstances we will never be led to assign a temperature number to it which is less than, say, —273. That is, there may be a lower temperature limit in the first sense, but not in the second. Also, it is possible that there should be no lower temperature limit in the first sense, but nevertheless a lower temperature limit in the second. This would be the case where the coldest possible object is assigned the temperature number  $-\infty$ . If an object at absolute zero could be produced, and we used a logarithmic correlation principle, this would be true. Now I wish to argue that there is a similar distinction to be drawn between different senses of the question: "Has the universe a beginning in time?". In one sense it is the question: "Is there a maximum age for events?". That is: "Is there, for whatever reason, a lower limit to the number which can be assigned to an event as the date of its occurrence?". But in the second sense it is the question: "Is there an event before which no other events occurred?".

Now it seems to me that these two questions have always been taken together as though they were one and the same question. But there is, I think, no contradiction in saying "Yes" to one and "No" to the other. It is logically possible that the universe should have a beginning in time in one sense, but not in the other. That is, it is logically possible that there should be no theoretical limit to the number which may be assigned to an event as its age, but that nevertheless there should be a first event. And also it is logically possible that there should be a theoretical limit to the age numbers of events, and at the same time that there should be no first event.

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<sup>5</sup> *Principien der Waermelehre*, Chapter 3.

This now seems to me so obvious that I wonder why it has not been commonly accepted. I think the answer must be that we all, naturally, think of time from the time-keeper's point of view. And dates are thought of as dependent on time-keeping. We thus fail to see that prehistoric dating depends very little on time-keeping. It depends upon the discovery of temporal properties and the adoption of a principle of correlation between temporal property and age—in almost, but not quite the same way, as temperature measurement depends upon the discovery of thermometric properties and the adoption of suitable principles of correlation. When we adopt the latter point of view, this ambiguity in the question "Has the universe a beginning in time?" becomes almost self-evident. When, however, we fail to adopt it, and adopt instead the point of view of a recording angel, the ambiguity mentioned becomes unintelligible. It becomes nonsense to suppose that the age of the universe is infinite but that there was nevertheless a first event; and nonsense to suppose that there was no first event but that the age of the universe is finite. For, from a God's-eye viewpoint, either there was a first event or there was not. If so, then the age of the universe cannot be infinite—for even God cannot count an infinite number of events. If not, then the age of the universe must be infinite.

But for whatever reasons this distinction may have been overlooked, it *has* been overlooked, and it is of the first importance. It is important, because once this distinction is recognised, it becomes clear that the question "Has the universe a beginning in time?" may be either of two distinct questions:

1. Is there a theoretical maximum to the age number which can be assigned to an event?

2. Was there an event before which no other events occurred?

The answer to the first question will depend upon an examination of our prehistoric dating techniques, upon the kinds of temporal properties employed, and upon the choice of extrapolated principle of correlation. The second question is, I think, in principle undecidable. Not even a recording angel, if there were such a being, and he were part of the universe, could decide this question.

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## THE PROBLEM OF COUNTERFACTUALS\*

By R. S. WALTERS

In most fields of investigation, some parts of the content of knowledge are expressed in the form of conditional statements that have been called counterfactual, contrary-to-fact, or (and this is sometimes taken to be a term of wider scope) subjunctive conditional statements. The following are examples of this kind of knowledge, although they are not exhaustive:

(a) Conjectures about possible historical developments, given as an initial condition some supposed historical event, incompatible with what actually was the case. Such conjectures, if true—and they must be true if they form part of the content of historical knowledge—"must be considered along with all other true and relevant opinions in any reasonable discussion" of certain actual historical developments or of policies proposed concerning certain actual events. One conditional of such a kind is, "If we had adopted a different policy towards Germany in the 1920's, the Second World War would not have occurred".<sup>1</sup>

(b) Conditionals derived from statements about dispositions. In order to understand the sense of any statement ascribing a dispositional predicate to a term and to agree that such a statement is true, we need to understand and agree to be true certain counterfactuals. This can be illustrated by considering an explicitly dispositional predicate, such as "soluble". If it is known that this piece of sugar is soluble, then it is known that, if this piece of sugar had been placed in water at any specific time, it would have dissolved. It would also be known that, if it were to be placed in water, it would dissolve. Conditional statements of the last kind are sometimes regarded as not counterfactual on the ground that the falsity or non-fulfilment of the antecedent is not presupposed in their being asserted. They do, however, raise similar problems to those of conditionals normally recognized as counterfactual and suggest, indeed, some questioning of the description of conditionals as counterfactual on the ground that their antecedents are in fact false or unfulfilled.<sup>2</sup> (I discuss this below, p. 33f.)

The example of a dispositional term is one of those which Ryle calls<sup>3</sup> "highly specific or determinate". Dispositional terms of a highly generic or determinable kind do not so readily sustain,

\* I am indebted to Professors J. L. Mackie and J. B. Thornton for criticisms of an earlier draft.

<sup>1</sup> R. M. Chisholm: "The Contrary-to-Fact Conditional", *Mind*, LV, 1946. Reprinted in Feigl and Sellars: "Readings in Philosophical Analysis", N.Y., 1949, p. 483.

<sup>2</sup> Cf. R. M. Chisholm: op. cit., p. 487, and K. R. Popper: "The Logic of Scientific Discovery", London, 1959, p. 434.

<sup>3</sup> "The Concept of Mind", London, 1949, p. 118.

if they do sustain at all, the derivation of counterfactuals. This is merely to say that the explication of highly specific dispositional terms is different from, and simpler than, that of the highly generic ones. For the present purposes it is sufficient that, in some cases, knowing that a dispositional term is truly predicated of something involves assenting to certain counterfactual conditionals.

(c) Conditionals derived from scientific laws, particularly those claimed not to have existential import: that is, scientific laws which contain such terms as "bodies which are freely falling, or at absolute zero, or in a perfect vacuum".<sup>4</sup> In cases of this kind, although one may not expect to find, for example, bodies which are freely falling, certain counterfactuals of the kind, "If body X were freely falling, it would do Y", are nevertheless accepted as true. In fact, one of the things taken to be puzzling, but nevertheless of importance, in cases of this kind is that the subject terms of the laws in question are thought to be not merely states of affairs that have not occurred, but states of affairs that are empirically impossible. The laws are themselves equivalent to such statements as "If there were any freely falling bodies, then . . .", or "If there were any gases with molecules of zero-extension, then . . .",<sup>5</sup> when it is the case not merely that there are not such states of affairs but that there could not be such states of affairs. The impossibility here is not logical, but something less than this: the world is so constituted, it might be put, that there could not be such states of affairs.

In this respect, case (c) conditionals might be thought to raise problems different in kind from those raised by case (a) and (b) conditionals, because these latter do not raise issues of empirical impossibility. The states of affairs conveyed in their antecedents are empirically possible, although the antecedents are as a matter of fact false. Such states of affairs could have been naturally expected to occur, whereas the states of affairs conveyed by the antecedents of (c) could never actually be expected to occur.

I do not think that this is even true of all cases of propositions, described as natural laws, with subject terms that do not exist. If the proposition, "All ravens surviving in snowy regions are white", were regarded as a law and there were no such ravens, it would not be immediately plausible to suggest that it is empirically impossible for there to be such ravens. It might, however, given further knowledge, be held properly that their occurrence is empirically impossible. By this, I mean that

<sup>4</sup> R. M. Chisholm: *op. cit.*, p. 487.

<sup>5</sup> An example from R. B. Braithwaite: *Scientific Explanation*, Cambridge, 1953, p. 298.

zoological investigations could discover some law or set of laws with which the proposition, "There are ravens surviving in snowy regions", is incompatible. This is to say that empirical impossibility, like other kinds of impossibility (and, one can add, possibility and necessity), is determined relatively to some set of propositions: in this case, of propositions accepted as laws.

It would, however, be granted that we have no such reasons now for believing it to be impossible for ravens to live in snowy regions; and if none do, then their not doing so is sheerly a matter of fact, empirically possible, but not actual. So far as the problem of counterfactuals is concerned, empirical impossibility raises no special problems, and case (c) is, in general, of the same kind as cases (a) and (b).

There may be a problem when, in the body of propositions making up a science, the subject term of a proposition accepted as a law is empirically impossible relative to some other proposition in that science also accepted as a law. Even this, however, does not seem to be a problem of any great difficulty.

Without going into the matter, I suggest that, when any proposition accepted as a law contains such a term, that proposition will be a derivative law in that science: it will be "not merely the generalization by itself, but . . . the generalization in the context of an established deductive system in which it appears as a deduction from higher level hypotheses which have been established independently of the generalization". This is a quotation from Braithwaite,<sup>6</sup> and although he is discussing here W. E. Johnson's well-known "brakeless train",<sup>7</sup> something which is empirically possible, his general comments are applicable to any case where a term is in some way empirically impossible. Whatever propositions concerning the danger of brakeless trains that we agree to be true are derived from higher-level hypotheses such as these: "that momentum increases with mass and velocity, that bodies with large momenta moving against only small retarding forces are dangerous" and so on. To this I should add that these propositions would still be true, even if there were also in the body of the science concerned a set of propositions, compatible with these "higher level hypotheses", that implied that no trains are (could be) brakeless.

#### *The meaning of the term "counterfactual"*

The terms, "counterfactual", "contrary-to-fact", and "unfulfilled", as qualifying "conditional statements", are normally thought of as equivalent. Conditionals so described have been thought to

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<sup>6</sup> Op. cit., p. 305.

<sup>7</sup> *Logic*, Part III, p. 12.

be counterfactual because the antecedent is counterfactual or false. It has also been supposed that a further peculiarity of such statements is that, when true, they express some "necessary connexion" between antecedent and consequent, independent of the truth or falsity of the antecedent and consequent. This is often taken as a causal or nomic necessity; but what I want to argue is that this "necessary connexion" is a concealed or assumed logical necessity, and that this introduces another possible meaning of "counterfactual". In its second sense, the term is co-extensive with what certain philosophers have called "subjunctive conditional statements", where this has been assumed to apply to statements such as, "If  $X$  were to be  $Y$ , it would be  $Z$ ". In this sense, it is wider in scope than it is in its first sense, since the antecedent need not be false.

The two senses of "counterfactual" are, then, (1) "factually false", and (2) "independent of fact". The second may be an unusual interpretation, but it is one that will bear investigation.<sup>8</sup>

(1) *Factually false*. In the case of contingent categorical propositions, the description of them as "counterfactual" is most appropriate when they are as a matter of fact false. The proposition,  $p$ , is counterfactual when  $\neg p$  is the case. In the case of conditional propositions, this is not necessarily so. To say that a conditional proposition is counterfactual is not to say that it is false in fact: indeed, in most of the cases discussed the conditional is thought to be true and discussion has tried to discover what its being true could consist in. Thus "counterfactual" has not been used to mean "factually false conditional", but rather "conditional with a factually false antecedent".

(2) *Independent of fact*. There is a possible sense of the term, although I do not believe that it has been intended in this sense at any time, in which one could describe certain conditionals as counterfactual, without thereby committing oneself to any belief about the truth or falsity of the antecedent. In this sense "counterfactual" is taken as equivalent to "non-factual" or "other-than-factual". To describe a conditional statement as counterfactual in this sense, then, would be to say that it is a non-factual or other-than-factual statement, namely, that it is a formal implication.

The sort of conditional statement that is normally discussed as counterfactual is not, as it stands, a formal implication, but could only be so considered along with certain assumed premises. I think that it is possible to hold consistently that what are called "counterfactual conditionals" can be so considered, and that the

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<sup>8</sup> Professor J. B. Thornton first suggested it to me.

"necessary connexion" of antecedent and consequent is but the concealed logical necessity of an argument.

*An account of what it is for a counterfactual to be true or false*

When it is said that some counterfactuals convey part of the content of knowledge, it is usually meant that what is conveyed is in some sense empirical or scientific knowledge. There are conditionals such as "If this had been a square, it would have been four-sided" and "If this had been a square, it would have been twelve-sided" that (given that the "this" in the second is not a twelve-sided figure) are straightforwardly logically necessary or logically impossible; but these are not thought to raise any serious problems. Only those that are thought, in some sense, to contribute to our understanding of the world are thought to raise problems.

These counterfactuals, since they convey part of the content of knowledge of the world, must be true; and, since, presumably, not all counterfactuals are true, it must be possible to indicate how those which are true differ from those which are false.

### (I) *True as material conditionals*

If counterfactual conditionals were always what I have called conditionals with counterfactual antecedents, then the following statements would be true:

- (i) any such conditional, taken as a material conditional, is true, since  $p \supset q$  is true when either  $p$  is false or  $q$  is true;
- (ii) the logical relation between any such conditional and another with the same antecedent but the contradictory of the consequent will be subcontrariety.

Consider the following pair of conditionals and assume that the common antecedent is as a matter of fact false:

- (a) If ravens had survived in snowy regions, they would have been white;
- (b) If ravens had survived in snowy regions, they would have been black.

Let (a) be written as " $A \supset C$ " and (b) as " $A \supset \text{not-}C$ ".

Some restatement of the original antecedent and consequent in the indicative rather than the subjunctive mood will be necessary here, but, given this restatement, such conditionals will be true when either  $A$  is false or  $C$  true, and false only when  $A$  is true and  $C$  false. Given  $A$  as false, then  $A \supset C$  and  $A \supset \text{not-}C$  are both vacuously true.

Anyone troubled by conditionals of this kind with false antecedents may admit that, taken as material conditionals, they

are vacuously true and that a pair of the kind given are in subcontrary relation; nevertheless, he may still claim that such details as these are irrelevant to the questions that interest him.<sup>9</sup> The fact that A is false is of no consequence to him whatever. He wants to disregard this contingent fact and to establish the consequences of A's being true, whether or not A is in fact true. Whatever these consequences may be, it should be clear at once that the consequences of A's being true are not purely logical consequences of A, but the logical consequences of A together with certain other propositions implicitly assumed or not yet specified. "Ravens have survived in snowy regions", if this is regarded as A, has few logical consequences, and none of them could in any way determine whether or not those ravens surviving are white. This would have to be determined on quite other grounds. His problem is, then, one of showing which of C or not-C is the case, on a supposal that A is the case. This amounts to the problem of saying which of two corresponding conditionals of types (a) and (b) is true and which false. The assumption here is that they cannot both be true and if, interpreted as material conditionals, they could both be true, then something other than this vacuous truth is required.

### (II) *Another interpretation*

If a counterfactual is true or false, it is reasonable to expect that it will be either necessarily true or false (a matter of logical necessity) on the one hand, or true or false as a matter of fact, on the other. Counterfactuals of the kinds in question do not clearly seem to be one or the other. It seems foolish to suggest that, if true, they are necessarily true, and equally that they are true just as a matter of fact. Nevertheless I think that both of these notions are included in the truth of any counterfactual. The view that I am maintaining is that the claim that a counterfactual conditional is true is equivalent to two other claims:

- (i) that a set (S) of contingent propositions is true as a matter of fact; and
- (ii) that this set (S) together with the antecedent (A) entails the consequent (C).<sup>10</sup>

The claim that a given counterfactual is false is equivalent to a denial of (i) or (ii) or both.

Anyone asserting a counterfactual commits himself in these

<sup>9</sup> As W. M. Kneale claimed. See "Natural Laws and Contrary-to-Fact Conditionals", reprinted from *Analysis*, in *Philosophy & Analysis*, ed. M. Macdonald, p. 228.

<sup>10</sup> Nelson Goodman in *Fact, Fiction & Forecast*, London, 1954, p. 18, presents such a view, having discussed the criteria of the set S with great subtlety.

two ways. The conditional statements called counterfactual are elliptical statements, when not used truth-functionally, and the consideration of such conditionals will make explicit the contingent or factual assumptions or presuppositions made, and show whether or not the implicit inference that has been thus made explicit is valid or invalid. The determination of the truth or falsity of a particular conditional of such a kind will depend upon the discovery of relevant factual propositions, whatever these might be. The determination of whether a particular assertor *knows* that his counterfactual is true will depend upon whether he knows certain relevant factual truths. One fact which is relevant to whatever might be thought problematic about counterfactuals is this: that no conditional called "counterfactual" generates or implies the contextual assumptions or presuppositions which help to determine its truth or falsity.<sup>11</sup>

Any conditional, asserted counterfactually or subjunctively, is, I am claiming, an implicit inference. "If ravens survive in snowy regions, they are white" differs in meaning from "If ravens had survived in snowy regions, they would have been white" and "If ravens were to survive in snowy regions they would be white", in the way which "X must be Y" differs in meaning from "X is Y". Just as the modal "must" (if it is not, say, merely an emphatic way of asserting an A proposition) contains an implicit reference to an argument, and so to unstated premises, so a counterfactual conditional, because of its use of the subjunctive mood, contains an implicit reference to an argument and so to unstated premises. The meaning of a counterfactual is not clear until such premises are made explicit. The grounds upon which such a conditional as "If ravens survive in snowy regions they are white" might be asserted are irrelevant to its meaning. But in the case of conditionals of the kind "If ravens survive in snowy regions they must be white", and "If ravens had survived in snowy regions, they would have been white", the grounds are necessary to their meaning, because their meaning is in each case that of an argument or inference with unstated premises.

R. M. Chisholm argues<sup>12</sup> that the meaning of a counterfactual ought not to be confused with the grounds upon which it is asserted. Asserting, for example, "If A, then C" (counterfactually), we may not know the grounds of its truth, but be merely conjecturing that there is some set of true propositions,  $S_1$ , guaranteeing its truth. But we understand the meaning without

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<sup>11</sup> Normally, when counterfactuals are spoken of, it is the conditional statements only that are referred to. I am suggesting that this is a loose use of the term and, strictly used, it should refer to the complete argument. I fear that I have not been consistent in adopting the strict use of the term.

<sup>12</sup> Op. cit., p. 490.

knowing anything about  $S_1$ . I do not think that this is the case, although it seems at first sight that what Chisholm is saying is intuitively right. The same kind of immediate appeal to intuition could be made about the relation holding between "If A, then C" and "If A, then not C" (both asserted counterfactually), and here it would suggest that these two statements could not be true together. This is not, however, supported by a logical analysis of possible cases.

It may be said that, in asserting "If A, then not C", I am again conjecturing that there is a set of true propositions,  $S_2$ , which guarantees the truth of this conditional. In certain cases when A is false, it is the case that "If A, then C" is true (given  $S_1$ ), and "If A, then not-C" is true (given  $S_2$ ).<sup>13</sup> Suppose for example that both

- (P) All ravens are black, and
- (Q) All surviving in snowy regions are white,

are true; and, further, that it is false that ravens survive in snowy regions. If X asserts that, "If ravens had survived in snowy regions, they would have been black", and assumes (P), then his counterfactual is true. If Y asserts that, "If ravens had survived in snowy regions, they would have been white", and assumes (Q), then his counterfactual is true as well. This is a result surprising to our intuitions of the sense of these statements, only if in our intuitive understanding we neglect their character as truncated arguments. I think that the claim that we can understand "counterfactuals" independently of the grounds on which they are asserted and the claim that two counterfactuals of the kind mentioned are in contrary relation are variant statements of the same theory of counterfactuals, and a demonstration that two conditionals of the kind given may be true together is at the same time a demonstration that we do not understand the meaning of given "counterfactuals" without knowing the grounds on which they are asserted. We do frequently understand what people mean when they assert "If A, then C" (counterfactually) because we supply satisfactory premises ourselves which are the same as those assumed by the assertor. Otherwise we may misunderstand by assuming premises not assumed by him or we understand the statement as one awaiting completion, with, perhaps, some views of our own about possible ways in which it might be completed.

#### *Consideration of an objection to this second account*

The account that I have here given is in accord with the conditions set down of the truth of a counterfactual. X and Y

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<sup>13</sup> Cf. K. R. Popper: "On Subjunctive Conditionals with Impossible Antecedents", *Mind*, LXVIII, 1959, pp. 518-519.

may both be asserting true counterfactuals, but they can do so only if the common antecedent is false. There may, however, still be serious dissatisfaction expressed at this view. Someone may say that the view expressed is quite beside the point and the question has not been squarely faced. There is some sense, such a person may say, in which X and Y cannot both be right, even though their common antecedent is false: there must be something further that we can discover showing that one is right and the other wrong. What has to be considered is what follows, not when we know that A is false, but when we *suppose* that A is true. Even granted that counterfactuals are incomplete until the assumed premises are made explicit, one set of premises must be in some sense less true than the other.

Clearly, there is a problem here. In the case considered, since the consequents of X's and Y's conditionals are incompatible, the supposal that A is true means that either (P) or (Q), the propositions presupposed by each, would have to be false. Formally the position is this: the following set of propositions comprises the premises from which X and Y deduce their incompatible consequents:

- (P) All ravens are black;
- (Q) All surviving in snowy regions are white; and
- (A) There are ravens surviving in snowy regions.

This set forms an inconsistent triad,<sup>14</sup> so, given that (A) is the case, one of (P) or (Q) must be false.

A precisely similar kind of situation exists with all counterfactuals. The examples, "If Hume and Voltaire were compatriots, then Hume would be French", and "If Hume and Voltaire were compatriots, then Voltaire would be Scottish", could both be true, given that X, asserting the first, assumes "Voltaire is French", and Y, asserting the second, assumes "Hume is Scottish". In this case the combined premises again form an inconsistent triad:

- (P<sub>1</sub>) Voltaire is French;
- (Q<sub>1</sub>) Hume is Scottish; and
- (A<sub>1</sub>) Hume and Voltaire are compatriots.

The inconsistent sets of propositions derivable from pairs of counterfactuals similar to those given would be in many cases more complex.

If one were told to assume that (A<sub>1</sub>) is true, and then to determine which of (P<sub>1</sub>) or (Q<sub>1</sub>) would have to be false, consternation would be justified. (P<sub>1</sub>) and (Q<sub>1</sub>) do not lend them-

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<sup>14</sup> This point and the substance of the argument arising from it were suggested by Professor J. L. Mackie.

selves to determinations of this kind. Assuming ( $A_1$ ) does not provide an occasion for rationally deciding between ( $P_1$ ) and ( $Q_1$ ). On the other hand, most philosophers would agree that the similar question asked about the set ( $P$ ) ( $Q$ ) ( $A$ ) is not so clearly undecidable. Assuming ( $A$ ) does provide an occasion for rationally deciding between ( $P$ ) and ( $Q$ ), and the attempt made by some philosophers to distinguish nomic universals or law-like generalisations from accidental generalisations is an attempt to enable the specification of those universal propositions in inconsistent sets such as ( $P$ ) ( $Q$ ) ( $A$ ), which would have to be true or have to be false if propositions of the kind ( $A$ ) were true. In most, if not in all, cases in which it is regarded as a sensible undertaking to decide, in a given inconsistent set of propositions, which must be false if the antecedent common to two counterfactuals is supposed true, the issue is likely to be between a supposedly law-like and a supposedly accidental generalisation.

In the example given, there would be little hesitation in saying that, given ( $A$ ) as true, then ( $P$ ) is false, but the reasons for this do not consist in any formal difference in the kinds of universal that they are, but rather in the relations which they have to other propositions in the body of science.

#### *Law-like and accidental universal propositions*

The discussion concerning law-like and accidental universal propositions, where this distinction has been supported, has normally sought for some criterion by means of which these two may be distinguished.<sup>15</sup> One criterion advanced is that law-like propositions sustain counterfactuals whereas accidental ones do not. "All surviving in snowy regions are white" sustains the counterfactual, "If  $X$  (whatever  $X$  might be) were to survive in snowy regions,  $X$  would be white". On the other hand, it is said, a proposition of the kind "All my friends speak French" will not sustain the counterfactual, "If  $X$  (whoever  $X$  might be) were one of my friends, he would speak French".<sup>16</sup> But this will not do as a criterion for distinguishing the supposedly different kinds of proposition, since on one view of what "sustains" might mean, any true universal proposition sustains a counterfactual, in the sense that the counterfactual so derived is in each case true, in accordance with the conditions set down above. Thus, I should argue, these two propositions could sustain counterfactuals in this sense. If it is urged against this that one sustains a counterfactual

<sup>15</sup> E.g., W. M. Kneale: *Probability and Induction*, Oxford, 1949, p. 75. See also "Natural Laws and Contrary-to-Fact Conditionals", reprinted from *Analysis in Philosophy and Analysis*, ed. M. Macdonald, p. 228.

<sup>16</sup> A Popper example from "A Note on Natural Laws and So-called 'Contrary-to-Fact' Conditionals", *Mind*, LVII, 1949, p. 64.

and the other does not, by which it is meant that one counterfactual is true, whereas the other is not, then the "true" counterfactual must be true in some stronger sense than I can allow. Granting for the sake of argument that there might be such a strong sense of "true", I can only say that the difference in kind between two universals would have to be known before it could be said whether or not one of them "sustains" a counterfactual in this sense, and thus the sustaining of counterfactuals could not be used as a criterion for distinguishing law-like universals from accidental ones. My own argument, however, is such that I cannot grant that there might be such a strong sense of "true".

On the account that I have given, both universals would "sustain" counterfactuals, since the process of sustaining counterfactuals is similar to the process of adding determinants to the subject term of universal propositions. "All X are Y" entails that "All X that are A are Y", whatever A might be. This is so if the subject term, X, is an unrestricted descriptive term, numerically neutral or of indefinite extension. If "my friends" were such a term, then the universal, "All my friends speak French" would sustain the counterfactual "If Confucius were one of my friends, then he would speak French". I should say that it does sustain the counterfactual and that any unrestricted universal proposition sustains counterfactuals. The dissatisfaction that is felt with this result is not a dissatisfaction with the logical procedure, but arises because incidental factual knowledge is brought into consideration. The fact that Confucius does not speak French is introduced into the logical context, providing another set of inconsistent propositions:

- (a) All my friends speak French;
- (b) Confucius is one of my friends; and
- (c) Confucius does not speak French.

When it is said that a proposition such as (a) does not sustain counterfactuals, the error involved seems to be this: that (b), the antecedent, assuming it to be true, is thought to falsify (a), the proposition under consideration. It falsifies (a), however, only when conjoined with a proposition such as (c). If (c) is not explicitly given, then (b) would falsify (a) only if it implied or "presupposed" (c). It does not imply (c), since (b) and (c) are indifferent to each other, and it would not be plausible in most contexts to claim that (b)'s being asserted presupposed (c).

I take these arguments to be sufficient to dispose of any attempt to suggest that sustaining counterfactuals or making counterfactuals true in some strong sense is a way of distinguishing law-like from accidental universals. If there is to be such a

distinction of universals, then it must be made on grounds different from these. The possible grounds for arguing that some universal propositions are accidental and some are law-like seem to be two, and they are not necessarily appealed to together:

(1) The first is, that accidental universals are such that their subject term contains some implicit restriction or limitation of the numbers denoted. This view is that such propositions are the results of summative inductions. Their subject terms are not unrestricted descriptive terms nor are they numerically neutral and of indefinite extension. In this sense, it may be claimed that "All my friends speak French" contains some kind of implicit restriction of the number of my friends: in other words, that the proposition itself is equivalent to a finite conjunction of singular propositions of the form, "X is my friend and he speaks French". The proposition is not explicitly so restricted, and it would need to be *shown* that it is implicitly so. There is no sense in calling such a proposition "accidental", unless this kind of restriction can be shown, or unless the following second sense is intended.

(2) The second is that those universals are accidental for which, although the subject term is an unrestricted descriptive term, and so number-neutral, no reason can be found in a universal proposition of wider scope.<sup>17</sup> In other words, the universal in question cannot be deduced from a universal proposition of wider scope, together with certain other propositions. On this view, "All ravens are black", although its terms are unrestricted descriptive terms, does not follow from any wider principle or universal. Indeed, in this case, a universal of wider scope (some expression of the principle of natural selection) apparently justifies or provides a reason for a contradictory of that proposition, although that contradictory may not be true in fact.

When, as here, two true propositions are thought of as in some sense incompatible, it can only be by considering them together with some other proposition that is false as a matter of fact but is such that, if it were true, then one or other of the original propositions must be false. In fact, it could be said generally that, for any categorical proposition, *p*, which is as a matter of fact false, at least two other propositions, *q* and *r*, could be found such that the set *p q r* is an inconsistent triad. Thus, in such a set, if *p* is supposed true, then either *q* or *r* is false. In some cases, as suggested, no rational grounds for preferring one of these rather than the other could be given. In some other cases, there would be rational grounds for preferring one propo-

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<sup>17</sup> This clearly will not do for any laws claimed to be ultimate. Their non-accidental character might be established by the number and variety of laws that they justify. Cf. R. B. Braithwaite: *op. cit.*, p. 300 ff.

sition rather than the other. Where  $q$  and  $r$  are universal propositions, the proposition rejected may be described as accidental. Such a description of it would accord with the second account given of this term, and would be a way of referring to its comparative isolation from the network of propositions in a given science or field of investigation. In contrasting it with another universal proposition from the same field said to be law-like, one would simply be drawing attention to its narrower range of crucial logical relations with the propositions of the science of that field. In other words, one would simply mean that, should such a proposition be found to be false, fewer revisions of the body of the science would be necessary than if one of those claimed to be law-like was found to be false. Any account of the sense of "accidental" and "law-like" stronger than this, for example one which sets up two grades of being universal, will not do.<sup>18</sup>

A decision concerning propositions in an inconsistent set such as the set  $p \ q \ r$  or (A) (B) (C) above will always be made, if it is made rationally, on the basis of preferring that proposition most strongly supported by the body of propositions in a science: in other words, that proposition which, if false, would require the more radical revision of that science.

#### *Decidable and undecidable counterfactuals*

The occasions for the use of conditional statements normally regarded as counterfactual are numerous, but the assent accorded to such statements can only be accounted for as the assent accorded to presupposed contextual propositions which are thought to entail the consequent as suggested. This assumes some common ground between assertor and hearer, some agreement that such and such are the contextual propositions. This assumption is sometimes warranted and sometimes not; but in any case of doubt there is scope for explicit statement of the contextual propositions. It is not, I maintain, until these contextual propositions are stated that the counterfactual is complete and the question of decidability arises.

"If X had been Y, it would have been Z", as a form of statement, is an incomplete counterfactual, and is similar to enthymeme and other arguments with suppressed premises. Given an enthymeme of the form, "A is B; therefore, A is C", we are provided with an incomplete argument which allows various possibilities of completion or interpretation; that is, various possi-

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<sup>18</sup> Cf. K. R. Popper: *The Logic of Scientific Discovery*, London, 1959, pp. 427-430, and R. B. Braithwaite, *op. cit.*, p. 295. A discussion of this matter with which I agree is given in R. Brown and J. Watling's: "Counterfactual Conditionals", *Mind*, LXI, 1952, pp. 226-231. Their general account of counterfactuals, however, is different from mine.

bilities of supplying premises that result in a valid or invalid argument. It may, of course, be complete as it stands (in which case it would not be an enthymeme) and so be formally invalid. Until further information is given, namely, that the argument is complete as it stands, or that such and such premises are "understood", the argument is undetermined as valid or invalid.

The use of the subjunctive mood in a conditional statement indicates that such statements are elliptical arguments. Anyone asserting such a statement implicitly conveys to the hearer a guarantee that he can provide a set of contextual propositions which establish his counterfactual as true in the sense already suggested. This guarantee is often spurious and the statement merely the expression of partiality or of a wish: the assertor has in mind no set of contextual propositions, or the propositions which he has in mind are some justification for the consequent, but they do not, together with the antecedent, entail the consequent. Such counterfactuals (and by this I mean the completed statement) are decidable, and would, on my account, be decided as false.

Mr. Hampshire, however, has claimed<sup>19</sup> that many counterfactuals are in principle undecidable, while many others are in principle decidable. He makes this distinction in terms of the content of a subjunctive conditional statement, and the distinction is one which coincides with the distinction between non-scientific and scientific fields. In the non-scientific field, such as law, history or morals, subjunctive conditionals are used which are in principle undecidable, which express "judgments or interpretations of the facts".<sup>20</sup> In the scientific field, since "the language of science (by definition) consists wholly of decidable statements", subjunctive conditionals are decidable as true or false. One consequence of this is that the question of whether borderline or disputed enterprises are scientific or not can be answered by determining how their subjunctive conditionals are to be considered.<sup>21</sup>

On the face of it, there seems to be some case for saying that conditionals such as, "If this metal had been heated, it would have expanded" are in principle decidable; and there seems to be no equally strong case for saying that conditionals such as, "If Hitler had invaded in 1940, London would have fallen" are in principle decidable. This is, however, misleading: each statement is, as it stands, incomplete and, according to the view I have

<sup>19</sup> "Subjunctive Conditionals", reprinted from *Analysis in Philosophy and Analysis*, ed. M. Macdonald, Oxford, 1954, pp. 207-8.

<sup>20</sup> *Ibid.*, p. 210.

<sup>21</sup> *Ibid.*, p. 208.

put forward, the question of decidability does not arise until the statement is completed. In the examples given, we should normally complete the first by the universal proposition, "All metals expand when heated", and, depending upon whether or not we regard this as true, we decide the counterfactual as true or false. We would, however, normally have no regular way of completing a statement of the second kind, and would await completion in whatever way it might be given. Once the statement is completed, I can see no reason why it could not be decided as true, in accordance with the conditions specified.

According to Hampshire<sup>22</sup> lawyers do not describe a counterfactual in a judgement or a legal argument as true or false: if they disagree with it, they dispute the "interpretation" of the facts which it conveys. Similarly, historians and moralists may disagree with the interpretations of facts conveyed in a counterfactual. In my view, however, they might, on certain occasions, be justified in saying that the counterfactual in question is false, or they might allow the counterfactual to be true, asserting another of their own which, while apparently contrary to the original, is true, in the way in which X and Y above asserted true counterfactuals in their apparently contrary claims. The natural place of the conditional statements in question in a historical exposition or a legal judgement is either at the beginning, where they anticipate a body of facts, however these might be selected, already "worked through", as it were, or at the end, where they succeed it as a summary statement.<sup>23</sup> Given this body of facts, as I say it should be given, as part of a counterfactual, then it makes perfect sense to say that this counterfactual is decidable. A counterfactual is only undecidable when for some reason its complete form is undiscoverable, or (and in this sense it might be claimed that counterfactuals incorporating natural laws are undecidable) when some constituent factual propositions are undecidable.

It might be claimed that, while all counterfactuals are excursions into logically possible worlds, nevertheless historical and legal counterfactuals make far greater excursions than those containing, for example, a single natural law and a statement of initial conditions, such as "If this metal had been heated, it would have expanded". This might be urged as a reason in support of their undecidability in principle. This reason, however, will not do.

It might be said, for example, that a judge who asserts that if Jones had not pulled the emergency cord, the accident would not have happened (and this is equivalent to his asserting that Jones's

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<sup>22</sup> Op. cit., p. 208.

<sup>23</sup> Cf. Hampshire: *op. cit.*, p. 208, and P. Gardiner: *The Nature of Historical Explanation*, O.U.P., 1952, p. 90.

pulling the cord is a necessary condition of the accident), or a historian who asserts that if Hitler had invaded in 1940, London would have fallen (and this is equivalent to his asserting that Hitler's invading is a sufficient condition of this state of affairs) could only defend their assertions by arbitrarily postulating certain conditions, by arbitrarily taking certain conditions as given. Given these arbitrary limiting conditions, it might be said, then the conditional could be decided but not otherwise: it is only in this artificial way that one can, in historical or legal matters, determine what would have happened as a consequence of a state of affairs which is contrary to fact.

These cases are, however, only different in degree and not in kind from the so-called decidable cases. When one claims that if this metal had been heated, it would have expanded, one assumes in addition to the universal proposition, a proviso of an other-things-being-equal kind. This proviso is a perfectly general one in such a case, but it could be made explicit in numerous ways. When, in the case of a historical or legal counterfactual, certain conditions are postulated as fixed or constant, this is, in my view, equivalent to making explicit what might be conveyed by the other-things-being-equal proviso. If someone wants to know what *really* would have happened, if a given actual event had not occurred, there is, of course, no reply that he can be given until we are told what precisely he wants to include in his possible world. What occurs in a possible world depends upon what one puts into that possible world, upon what one takes as fixed or constant in it, and considerable variety is possible in what one takes as constant.<sup>24</sup> Any belief that there must be something which is *really* the case when one makes a contrary to fact supposal, is based on the same grounds as the desire for a strong sense of the truth of counterfactuals, and according to my view there is no such strong sense.

Given that Voltaire is French and that Hume and Voltaire are compatriots, it is "really" the case in this possible world that Hume is French. Given that all ravens are black and that there are ravens surviving in snowy regions, then it is "really" the case in that possible world that those ravens are black. The counterfactuals of law and history are in my view of a precisely similar kind as these, their decidability, or, perhaps more strictly, their being true, depending upon which true propositions particular assertors incorporate in their possible worlds. Such counterfactuals may be slightly more complex in their constituents than

<sup>24</sup> When it is said that a lawyer disputes the judge's interpretation of the facts, it is this selection of what is fixed or constant that is disputed or objected to.

some others, although this need not be so; but the general conditions under which such counterfactuals are true are the same as those under which any counterfactuals are true.

### *Conclusion*

The outcome of my discussion is this: that, since the truth of counterfactuals, properly interpreted, is an amalgamation of factual truth and necessary truth (or validity of argument), then counterfactuals are of little importance in any field of knowledge. Certainly, they do not in any way advance knowledge or express mysterious more-than-factual connexions of facts: they only convey part of the content of knowledge by incorporating in their complex form, normally implicitly, matters of fact already known or assumed to be true. When counterfactuals with incompatible antecedents and incompatible consequents are considered, it is not the case that one expresses a merely accidental connexion of states of affairs and the other an essential or more-than-factual connexion. Whenever a decision is made between them, what is being decided is which of an incompatible set of propositions would be accepted as true, if the false antecedent were true. In some cases, as suggested, no rational decision could be made one way or the other. In other cases, the grounds upon which a decision is made are grounds of least logical disturbance to the propositions in the body of a science. This is a matter of the logical relations holding among the propositions of a science, and is best discussed without referring to the so-called problem of counterfactuals at all.

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## ON PHILOSOPHICAL ANTHROPOLOGY

By H. O. PAPPE

'On the continent of Europe', says Herskovits in his textbook, the term . . . 'anthropology is reserved for the study of physical type: cultural anthropology is not called anthropology at all'.<sup>1</sup> Yet if one surveys German and other Continental writing over the last thirty years, one finds a large number of books and articles dealing with anthropology in a sense quite different from that mentioned by Herskovits. These writers call their work 'Philosophical Anthropology' (PA hereafter). Far from accepting Herskovits' definition, many of them dispute the right of physico-ethnological anthropologists to use the term 'anthropology' to designate their work. According to them the original meaning of the term has, since the late sixteenth century, served to describe the basic knowledge of human nature and of the human condition. Conventional anthropology, in its search for details regarding man and societies, presupposes knowledge of what man really is. PA, however, questions this knowledge; it takes it as its very problem. It aims to elucidate the basic qualities and conditions which make man what he is and which distinguish him from all other beings. PA could then appear to have a well-defined task setting it off from other disciplines. However, it tends to operate as a co-ordinating system, which incorporates knowledge about man as evolved by other branches of the natural and social sciences. It thus aims to provide the master key for the understanding of man's position in the world.<sup>2</sup> One is reminded of the 'science of human nature' which Scottish and English philosophers have pursued since Hobbes, Locke and Hume; in particular of Hume's statement that 'in pretending to explain the principles of human nature we in effect propose a complete system of the sciences'.<sup>3</sup> However, as I shall endeavour to show, there are basic differences between this approach and the methods applied by many philosophical anthropologists.

<sup>1</sup> M. L. Herskovits, *Cultural Anthropology*, 1955, p. 8.

<sup>2</sup> Kant distinguished physiological and pragmatic anthropology. While the former treats of man's limitations as set by nature, the latter deals with man's potentialities, with 'what He, as a free agent, makes of himself, or is able and ought to make of himself'. *Anthropologie*, Vorrede, p. 1. (In *Die Metaphysik der Sitten*, part 2, introd. III, however, Kant contrasts empirical 'Naturlehre' (Anthropologie) with a priori morals (Sittenlehre).) According to H. Plessner, PA is to mediate between and co-ordinate physiological and pragmatic anthropology. *Zwischen Philosophie und Gesellschaft*, 1953, p. 121.

<sup>3</sup> *A Treatise of Human Nature*. Introduction (Everyman ed. Vol. I, p. 5).

## I

There is little to be found in English writing about PA, though Cassirer's *Essay on Man* belongs in its compass, and Collingwood's and Toynbee's thought shares important elements with it. On the Continent it has become a powerful movement and is, in fact, widely recognized as epitomizing the essential task of philosophy today. A look at concrete examples of PA will contribute to a better understanding of what it means. What sort of people actually do practise PA? Comparatively few are professionally in this line; there are only a few academic chairs of PA.<sup>4</sup> On the other hand, the number of professed adepts is surprisingly large, and so is their range over adjoining disciplines.<sup>5</sup> They are by no means restricted to Germany. There are many Swiss, Dutch, and French scholars amongst them. There are philosophers who attach primary or, at least in some way, fundamental significance to PA, such as Scheler, Plessner, Hartmann, Landsberg, Häberlin, Dempf. There are economists (Sombart), ethnologists (Frobenius, Jensen), historians (Ed. Meyer, Dilthey, Spengler, Rothacker, Litt), historians of art (Sedlmayr), sociologists (v. Wiese, Adorno, Gehlen, Freyer), theologians (Bultmann, Kuhlmann, Gogarten, Brunner), and there are biologists (Uexküll), zoologists (Portman), doctors (Weizsäcker), and psychologists and psychiatrists (Jaspers, Pfänder, Binswanger).<sup>6</sup> This is certainly not a complete list, but even so we are confronted with a baffling variety of research. We are not faced, as we might have expected, with the gradual growth of a new branch of science. It is rather that many scholars, well established in their own lines, found themselves within the pale of PA after its significance had been disclosed to them. We may think of Molière's bourgeois gentilhomme who was startled and proud to discover himself as speaking prose. It is natural to ask

<sup>4</sup> E.g. at Gottingen and at Nijmegen (the latter being a chair of psychology, anthropology, and pedagogics).

<sup>5</sup> Many philosophical anthropologists prefer to emphasize their peculiar perspectives in speaking of theological, historical, biological, phenomenological anthropology and culture-anthropology rather than of PA (which is, however, understood to permeate and co-ordinate these sub-divisions). Rothacker's *Kultur-Anthropologie* is not the same as American cultural anthropology though there are common traits.

<sup>6</sup> For an English language account see H. Wein, 'Trends in Philosophical Anthropology and Cultural Anthropology in Post-war Germany', 24 *Philosophy of Science* (1957), pp. 46-56; also David Bidney, *Theoretical Anthropology*, 1953, *passim*. A perceptive account of Kant's, Feuerbach's, Nietzsche's, Scheler's and Heidegger's anthropological theories is contained in part V of Martin Buber's *Between Man and Man*, 1947. Also Herder and Wilhelm von Humboldt belong to the 'founders'. Amongst the large number of historical and systematic contributions to the subject, Michael Landmann's textbook *Philosophische Anthropologie*, 1955, offers the most judicious and sober introduction to PA.

what can be the common denominator of this *embarras de richesse*?

When, in the English-speaking world, we nowadays speak of the modern 'revolution in philosophy', we think of philosophy having become more secular and more technical, largely preoccupied with the nature of philosophical enquiry and with the notion of meaning. On the Continent, and in Germany in particular, philosophers like to speak of 'Copernican turns' rather than of revolutions, and, since the twenties, there has been a fashion of talking of the 'anthropological turn' in philosophy. Philosophical anthropologists set out from the conviction that the theory of knowledge had reached a desperate crisis. Traditional theory of knowledge is seen by them as occupied only with one of the functions of consciousness;<sup>7</sup> and consciousness, in turn, is understood to present only a part of the forces shaping human reason (as distinguished, in the Kantian sense, from understanding). To PA knowledge is not autonomous but depends on dispositions and heterogeneous influences. 'The idea of logic gets dissolved in the maelstrom of basic questioning', as Heidegger put it in somewhat extreme terms. This type of argument is used by the vitalist and existentialist schools of anthropological thought. PA, furthermore, is opposed to leaving the investigation of the empirical world solely in the hands of the scientists. 'Toward the things themselves', and the 'turn towards the objective' are expressions which mark the phenomenological attempt at developing a peculiar method for analysing or, rather, interpreting facts, qualities, relationships, and the basic categories of human nature and culture. This objective approach, to be sure, does not imply the application of the methods used by the empirical physical sciences; it refers to the peculiar philosophical methods which should be applied, in particular the phenomenological method.

PA thus aims to overcome what its followers call the 'crisis of science'. The crisis of science is said to have been brought into view by the modern developments in biology and psychology, and by the establishment of the indeterminacy principle in nuclear physics. It is difficult to see why these, or any, scientific advances should have been considered as constituting a crisis in science. However, representative German philosophers since Kant had long maintained a conception of science as something fixed on the lines of a rigid mathematico-mechanical determinism. This

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<sup>7</sup> I.e. as leaving out of account what Pascal called the 'logique du coeur' or the 'esprit de finesse'. Hamann's, and especially Herder's philosophy, which rejected the 'vis rationalis' (the syllogistic reasoning faculty) as something partial and inadequate, is here a significant source.

basic conception, they feel, has broken down. There is rather wide consensus among Continental thinkers that the materialism of the nineteenth century has been overcome, and that the peculiar methods of the 'Geisteswissenschaften' have finally been vindicated. These methods are concerned with the significance imminent in events and in the works of men rather than with the causal nexus between events. Their aim is the interpretation of other minds (both individual and collective), of their peculiar motivations and tendencies as well as of the institutions in which their ideas have found expression. This 'hermeneutic' process is primarily directed towards the conscious and unconscious actions of human beings and towards the structure of interpersonal (social and cultural) relationships. It is conceived of as contrasted with the methodology of natural science. It is said to be descriptive-interpretative, organic, and concrete rather than explanatory, mechanical, and abstract. In short, instead of one logic or one methodology with a variety of rules, there is here a dichotomy resulting in two distinct methodologies, namely causal explanation and 'Verstehen'.<sup>8</sup> This attitude is not exactly novel. It rather takes up the theme of an older tradition in Continental philosophy, known in English thought as the Germano-Coleridgean school, with its emphasis, in Mill's words, on a philosophy of society in the form of a philosophy of history, and aiming at a philosophy of human culture.

The outstanding instance of the procedure favoured by PA is the phenomenological method which has been adopted by about half of all philosophers and an ever-increasing number of social theorists.<sup>9</sup> Rather than treating this procedure in detail I shall attempt to illustrate its operation by means of three examples of anthropological investigation. This should furnish a better taste of the dish, and it is also in keeping with the phenomenological method. I shall, however, say a few words about this method when we come to our third illustration.

My first example deals with the image of man in our own consciousness and why it should matter to us. I shall then review the meaning of technology in an anthropological perspective.

<sup>8</sup> I am over-emphasizing the contrast for argument's sake. There is some common ground between linguistic philosophy and Continental trends. For a recent example of British philosophy in a Continental key see P. Winch, *The Idea of a Social Science*, 1958. For an example of a detached treatment of the problem, taking into account both cis- and trans-Channel thought, see A. Carr-Saunders, *Natural Science and Social Science*, Eleanor Rathbone Memorial Lecture, 1958.

<sup>9</sup> I. M. Bochenksi, *Die Zeitgenössischen Denkmethoden*, 1954, p. 25. It may be appropriate here to recall the names of Brentano, Meinong, Husserl, all of whom have, more or less, left some mark also on British philosophical thought.

Finally I shall turn to psychology to show how applied anthropological method actually works.

## II

The self-image of man has become of central importance for various reasons. Science has permitted man to control nature with the following effect: Formerly man was up against man but primarily against nature in the form of wild animals, disease, hunger, cold, flood, poor soil, and so on. Today, nearly all these phenomena have been or can be brought under human control. Man is not threatened by his old enemy or by the God who made nature. He fears the man-made use of nature in the form of hydrogen bombs, etc. He is up against man, man-made structures or the God who made man. Finally even in the very picture which man (in his scientific representatives) has of nature today he meets himself rather than natural data. For the object of natural science is not nature as such any more, but nature as we question it for specific purposes and in the specific contexts of axiomatic frameworks determined by ourselves. The mathematical formulae which scientists use do not represent nature but rather our knowledge of nature. Man finds himself inescapably confronted by man.<sup>10</sup> We have every reason to ask this crucial question: what is this man? What, however, causes us to ask the question: in what form does man's subjective image of himself appear in his consciousness?

The self-image of man is understood by PA to be tremendously important. It determines what man makes of himself, into what sort of man he turns himself. Animals are as nature has created them. Man, however, is unique in that he must complete his character, for which nature has supplied only rudiments. He has to form his personality, and he does so according to the images in his mind of what he can and should be.<sup>11</sup> Seen in historical perspective man's consciousness of himself offers a pattern of five types, unfolding in progressive growth: from cosmological all-unity of animal and plant world, over the demarcation of man as the possessor of the Logos, as the child of God, to the pantheistic position of the late middle ages and of modern

<sup>10</sup> See W. Heisenberg, *Das Weltbild der heutigen Physik*, 1955, *passim*.

<sup>11</sup> I am using for this part an investigation by Scheler, which is considered to be one of the pioneer pieces of the anthropological discipline. M. Scheler, *Mensch und Geschichte*, 1926; re-printed 1954. Scheler's most important contribution to the subject is *Die Stellung des Menschen im Kosmos*, 1927; reprinted 1947. The concept of 'image' or of 'reality worlds', in a sense more or less related to that of Scheler's investigation, as well as an awareness of the typological variety of 'Weltanschauungen' has of late been widely taken account of in socio-psychological and political thought. Simone de Beauvoir's *The Second Sex* is a brilliant application of the procedure.

times with its *Cogito ergo sum* and *Cogito ergo mundus est*. An appropriate anthropological question here is: Is this a historical process of an increasingly deeper penetration towards the true human condition, or are we rather faced with symptoms of a megalomaniac disease?

Scheler's typology represents five answers to this question. His images of man are those of *homo religiosus*, *homo sapiens*, *homo faber*, *homo dionysiacus*, and *homo creator*. The first of these is based on the Christian-Jewish legacy of super-naturalism with its ensuing feelings of genetic guilt and awe—a legacy in which we all share whether we are believers or not. *Homo sapiens* conceives, in various forms, of the divine plan and of rational man as being in harmony with one another. The fleeting conception of a peculiar historical situation (culminating in the Enlightenment), this view has become dim in the light of another image of man, that of *homo faber*, a rather naturalistic or pragmatic type. Man is now seen as the highest-developed animal, the maker of tools, including language, in whom a particularly high proportion of animal energy is used in cerebral activities. This view overcomes the dualism of body and soul which are now seen as a functional unity. Human being and development are explained by the primarily urges of animal nature, i.e. the libidinous urge for progeny, the urge for power and growth, and the urge to possess and to eat. Indeed, the philosophies of history of Marxism, of racism, of Machiavellianism, and parts of Freudianism are, so it is claimed, based upon this interpretation of man. However, nearly all these naturalistic conceptions still share, with one another as well as with their predecessors, the belief in the unity of human history and the belief in a meaningful evolution in the sense of a development towards higher organisation (Marx, Comte, Spencer, Kant, Hegel, Darwin).

The fourth and fifth conceptions of man break with this tradition. They are both peculiar though radical phenomena of that intellectual situation which heralds a new orientation of anthropological thought. Our fourth picture is that of decadence immanent in human nature and history. With this we move decidedly over to the Continent, to the world of Schopenhauer, Nietzsche, Bergson, of Neo-romantics such as Klages, Spengler and Frobenius. Man, viewed in the light of this anthropology, is the 'deserter of life', the 'faux pas' of life; a megalomaniac species of rapacious apes; an infantile ape with a disorganised system of inner secretion. Compared with other animals, man is essentially deficient in vital powers. He is dependent upon technical means for his survival; and his much vaunted power of thought is an artificial surrogate for his missing or weak instincts. His 'freedom

to choose' is a euphemism for his lack of direction; his social institutions are pitiful crutches to assure the survival of the species which, in a healthy race, would be assured through biological vitality. The state and the law, replacing the instinctual urges of blood and soil, society, instead of community, are symbols of vital impotence. Reason is here regarded as separated from the soul which belongs to the vital sphere of the body; reason is the destructive, demoniac principle struggling with and submerging the healthy powers of the instinctual soul.

Our fifth image of man, that of *homo creator*, brings us even closer to the specific world of PA. This conception, too, is derived from the powerful influence of Friedrich Nietzsche. The idea of the superman has been transformed into a stricter philosophical system by Nicolai Hartmann and Max Scheler. Scheler calls this view a 'postulatory atheism of high responsibility'. We have no ontological knowledge of an ultimate being. However, contrary to Kant's and his predecessors' postulate of the desirability of God, there must and shall now be no God—for the sake of human responsibility and liberty. It is only in a mechanical, non-teleological world that there is room for a free moral being. Where there is a planning all-powerful God there is no freedom for man responsibly to work out his destiny. Nietzsche's 'God is dead' is understood, not in the sense of moral libertinism, but in the presence of an ultimate moral responsibility of man. The predicates of God (pre-destination and providence) are to be related to man. And mark: to man personally and individually, not to humanity, as in the case of Comte, nor to any other collective body. The effective power of the collective structures in history is not deified, as by Carlyle or Treitschke, but they are regarded as the background against which the genuine personality has to measure and prove himself. Those familiar with the existentialism of Sartre, Merleau-Ponty and Simone de Beauvoir will recognise these ideas and will appreciate the response to them outside Germany.

The anthropological significance of the self-image of man consists in the light it sheds on the central concern of PA: to illuminate the whole range of genuine potentialities in man so that his choice of a form of life should not be restricted by narrowness of view.

### III

Our next example has to do with the serious consequences which follow from the choice of a form of life too narrow to offer a balanced range of values. Expressed in anthropological terms, we are up against a mode of life modelled on the lines of a partial

anthropology which does not do justice to vital human needs. I am turning to a favourite subject-matter of philosophical anthropologists, the impact of science and industrialisation upon man and society.

The English-speaking world's attitude to technology is in general determined by practical problems caused by technical change. It finds expression in the attempts to bridge the cultural gap caused by alterations in what is an essentially manageable pattern. Some German sociologists of knowledge, on the other hand, hold that the unquestioning belief in the objectively valid world of natural science and technology is a flight into dogma (mechanical and mathematical dogma) of modern man lost in a world without religious allegiance. Indeed, there is a tendency to see demoniac forces confronting us in technology, forces which account for much of the evil we have experienced and which threaten to submerge Western civilisation. In a more playful way, one can discover such ideas in Samuel Butler's *Erewhon*, and one may also think of the views from Burke to Christopher Dawson and Elton Mayo. Many books and symposia have been published in Germany on this subject, the Jünger brothers being the notorious experts. Even so distinguished a sociologist as Alfred Weber has erected into a scholarly theory ideas and anxieties of the type which Aldous Huxley and Orwell made the subject of their literary speculations about the future. However, a philosophical anthropologist such as Gehlen develops a more moderate view, and I shall enumerate some of his more suggestive ideas.<sup>12</sup>

The use of technical means is inevitable for man as he is a vulnerable deficient being. He needs artificial weapons, clothing and tools to survive. And he needs them not only to make up for his deficiencies but also to ease the burden of toil. He also improves on nature by inventing things not seen in the natural world around him, such as certain methods of making fire, the bow and arrow, the wheel, movement by means of explosion, etc. This preoccupation of man with the improving of techniques causes him increasingly to turn towards things, i.e. towards the inorganic world. And, in turn, the occupation with the inorganic

<sup>12</sup> Arnold Gehlen, *Die Seele im technischen Zeitalter*, 1957. This is the most recent statement of Gehlen's position. Gehlen's main work is *Der Mensch, seine Natur und seine Stellung in der Welt*, 1940 (5th ed., 1955). Judging from the impact which he appears to have made on contemporary German philosophical and sociological literature, he may well be the most influential writer in German philosophy and sociology today. See also *Urmensch und Spärkultur*, 1956. Gehlen has certain points of contact with American thought, esp. that of George Mead and of William James (rather than that of such pragmatists as Peirce and Dewey). When Gehlen speaks of the individual and of individual freedom, he thinks in terms of Ihering's and Duguit's form of Organicism. This means that the individual is considered to be free only if he merges himself into the wider purpose of a group such as his nation.

leads him progressively into ways of thinking appropriate to the manipulation of the inorganic, into thinking by way of abstract models and mathematical concepts. One result of this has been tremendous (and cumulative) progress on the mechanical side while, at the same time, our understanding of the organic processes of life has hardly advanced beyond the stage of the Greeks. The intellect as we have developed it (one may think of Bergson) comprehends only inorganic acts. Yet the biological and spiritual spheres do not follow the same pattern; they do not respond to the causal-mechanical laws developed by natural science.

How then to arrive at a deeper or at least more comprehensive understanding of technology? It is possible and justified to emphasise the magical element in technology. There is a fascination for man in the automatic processes of nature. He constantly aims to understand his mysterious ego as reflected by the phenomena around him. He identifies himself with animals (Totem) and with shadows. His relationship to the machine is similar. There is a deep urge in man to express himself by means of rhythmic, periodic, automatic processes. Observe a young man on his motor cycle; the tremendous roar, in which he revels, the aimless speed are means of self-expression. The automation of the machine emerges in this view as a value irrespective of the cause-effect relationship. Mechanisation thus comes to be regarded, not so much as freely intended by sovereign rational planning, but as a blind natural process subconsciously developed.

Moreover, the magic impact of automatic processes contributes to the liberating effect of mechanisation. By means of organised, and therefore understandable, activity it liberates man from the paralysing fear (*Angst*), to which he is helplessly exposed in the face of nature. His instinctive urge to repeat routine actions results in his reducing the world around him to human proportions. The magic force of automation lies in its presenting a copy as it were of the closed structure of the organic human processes such as the regulation of blood pressure, and also of human action. Man applies this procedure of copying the life-processes in creating and controlling inorganic nature. He thus unifies and socialises technology, biology, physics, psychology and sociology,<sup>13</sup> and finally this process finds its climax in the discipline of cybernetics. This leads to a tremendous man-made universe. Yet it regulates only one aspect of human life. These

<sup>13</sup> The limitations inherent in the use of models and, in particular, of analogies, certainly present a genuine epistemological problem. Gehlen's argument, however, also requires to be understood against the background of a rejection (though not a consistent one) of rational behaviour as something de-vitalised and inferior, in cognitive strength and will power, to animalic, instinctive actions. See *Der Mensch*, 5th ed., 1955, pp. 328 et seq., 353, 381.

man-made copies of life processes do not permit us to come to a deeper understanding. They are of a mechanical nature and present only a functional aspect of a larger whole which greatly transcends this particular field. There is no substantial likeness, there is only structural parallelism.

The one-sided emphasis on mechanical structures has led to essential cultural changes. The instinctive subconscious logic, which has led us to use the seemingly highest conscious processes, has not only affected modes of social and economic life—it has also resulted in a mutation as regards the structure of human consciousness. The purposive or 'zweckrationale' mode of thinking has crowded out and has emancipated man from considerations of value. The experimental character which has come to prevail in thinking is by definition destructive of stable attitudes, obligations, and opinions. Traditional methods, such as family morals, become useless as the logic of the experiment takes over, and the old cultural variety gets replaced by stereotypes.

The structure of experimental thinking does not lead necessarily to a frontal attack on religion and morals. Science and technology are rather 'alienated' from 'Weltanschauungen'. Yet the new style of thinking brings about a substitution of method for substance—method applied irrespective of subject-matter, method for method's sake like *l'art pour l'art*. The aim pursued is not so much a particular social or moral achievement but to find out where the experiment will take us. There is a belief that substantive insights will somehow follow if only we use rational mathematical methods. The new structures in place of the old theories are determined by methods which have become so esoteric that only a small, sometimes a tiny, number of experts understand what is essential for the survival and progress of human society. A new style of thought develops which emphasises terms such as Norm, Standard, Inter-changeability, Momentum, Optimal Economic Effect. Look at politics, morals, and the arts today and you will appreciate the primitivisation which is the counterpart of an esoteric mechanical culture. Unless we consent to being doomed, there is a task for the discarded and outmoded intellectual leaders: this is firmly to demarcate the limits to which mathematical and natural science methods may be used.<sup>14</sup> This task of the cultural personality is, so it appears, not dissimilar from the

<sup>14</sup> However, Gehlen can see occasional merit in the mathematical approach. This is so in the case of psychoanalysis which, because it could be universally understood, is said to have exercised a dangerously wide influence, both in its popular appeal and its (unwarranted) impact upon social psychology. *Die Seele*, etc., p. 103. Views similar to Gehlen's in this context are held by other leading sociologists such as L. von Wiese in *Kölner Zeitschrift für Soziologie*, New Series 3 (1950/51), pp. 459-469, and H. Schelsky, *Soziologie der Sexualität*, 1955, p. 7 and passim.

rôle provided for philosophy by Whitehead in 'Science and the Modern World'.

#### IV

Our third example will be taken from the strongest group, the psychologists and psychiatrists. Countable by the hundreds, they are to be found in Germany as well as in Switzerland, Holland, and France.<sup>15</sup> They include brilliant names such as Jaspers, Buytendijk, Merleau-Ponty, Binswanger, and their thought is closely related to the philosophies of Dilthey, Husserl, Scheler, Heidegger, and Sartre. What is common to them is the view that traditional experimental psychology requires the assistance of philosophical thought to arrive at satisfactory results. Some of the philosophical anthropologists are radically opposed to the empirical hypotheses and inductive statistical methods of traditional psychology. Most of them, however, combine this traditional approach with their peculiar philosophical or phenomenological method. Their discipline, dealing as it does with individual cases, lends itself more readily to a descriptive approach. While many philosophical anthropologists preferably dwell on generalities, the psychological anthropologists (and the philosophers associated with them) have developed a body of detailed investigations such as analyses of what imagination is, or suffering, laughter, weeping, shame, resentment, pleasure, love, fear, and so on. When I speak of analysis I do not mean the analysis into qualities, elements, and functions based upon sensual perception of real things and events, i.e. that analysis which results in a theory and finally in a natural law. It is rather phenomenological 'existential analysis', as Ludwig Binswanger calls his new psychology, which takes its cue from Husserl and from a rather reluctant Heidegger. Binswanger, one of the leading Swiss psychiatrists, has developed his teaching against the background of Freud's psychoanalysis.<sup>16</sup>

Binswanger does not exclude the methods of natural science but he makes two objections to reveal its inherent limitations. One

<sup>15</sup> See S. Strasser, 'Phenomenological Trends in European Psychology', XVIII, *Philosophy and Phenomenological Research* (1957), pp. 18-34.

<sup>16</sup> Binswanger was associated with the rise of psychoanalysis from the beginning, though he never joined Freud's circle. He has recently published a volume of reminiscences of Freud and of correspondence with him. *Erinnerungen an Sigmund Freud*, 1956. In 1936 Binswanger, besides Thomas Mann, was invited to deliver the official address in celebration of Freud's eightieth birthday; he took the opportunity of outlining what separated his teaching from that of Freud. I have, in the text, made particular use of this lecture as well as of other papers contained in Binswanger's *Ausgewählte Vorträge und Aufsätze*, Vol. I (1947): *Zur phänomenologischen Anthropologie*. Binswanger's anthropological main work is *Grundformen und Erkenntnis menschlichen Daseins*, 1942. See also Freud's letter to Binswanger quoted in Jones Vol. III, p. 218.

is that all abstractions, even those of 'Gestalt' psychology, are transpositions and simplifications. The other is that they illuminate of necessity only part of the field of investigation, because they are based upon a one-sided philosophical anthropology; i.e. upon an image of man in the world which does not do justice to all the potentialities of man and thus ignores essential elements of diagnosis and cure. To make his meaning clearer, phenomenological perception is not registration of stimuli (as in traditional psychology), but it connotes selective, complementary, and synthesising activities. The perceiving subject constructs a meaningful whole out of the given raw material. We do not perceive sensual perceptions, but we perceive objects towards which we are intentionally directed. By reduction, i.e. by abstracting from subjective and conventional accidental properties, we progress to the intuition of the essential qualities. We perceive meanings which are not accessible to the discursive methods of the natural sciences.

The psychological investigation according to Binswanger's school is directed towards the inner life history of the patient, his self-structuring according to his inner sense or inner motivation. Self-structuring in this sense is equivalent to the response the individual makes to the challenge of the world around him; or to his character which develops in a dialogue with and as an integral part of his inner life history. Take the case of St. Augustine, to whom we owe the beginnings of individual autobiography. Illness prevented him from carrying out his ambition to become an orator. He overcame his disability by turning towards the spiritual world and thus arrived at his essential 'real being'. He could have reacted otherwise: by resentment or frustration, by neurosis, suicide, or rent psychosis (had he been a contemporary of ours). All these and other potentialities offered themselves; or rather the temptation to restrict his character by the impoverishment inherent in an irresponsible choice. But Augustine's answer was the choice of an autonomous life preserving to him access to the full range of human values.

The contrast between the methods comes out clearly in the case of 'heel-phobia'. A five-year-old girl lost the heel of a shoe when it stuck in the skate that was being taken off; she fainted. At the age of twenty-one she developed a phobia, reacting with escape or fainting whenever she sighted a loose heel or heard people talking of heels. Pre-Freudian psychology would have diagnosed that her phobia was caused by the event on the skating rink. Freudian psychoanalysis would rather fasten on the girl's phantasies, the fear of being separated from her mother and of losing a child. The procedure of existential analysis would be as

follows: Everyone suffers the trauma of birth, and many lose heels without getting hysterical. Why then has this particular individual person these phantasies? This is a matter of disposition, which is an anthropological category. It concerns our relationship to the world around us, i.e. our world image, and it concerns the threat which the situation offers to this image. In the case of heel-phobia the decisive category is that of continuity. The patient's world-image is dominated by this category. The event on the ice symbolises the threat of interruption of continuity, the threat of separation from mother and child. The terrifying fear or 'Angst' experienced by the patient is the fear of her world-image being threatened. This process is not conscious or need not be; nor is it subconscious. It is underlying and occasions subconscious reactions—an ultimate framework which may be compared with Kant's transcendental forms of human reason which make experience what it is. The illness is seen as a stereotyped world-image overpowering the patient and making him unfree to choose among the rich potentialities of being-in-the-world. And this leads us back to the very theme of contemporary philosophical anthropology: to establish a complete picture of the potentialities open to man.

Is the massive support commanded by PA amongst Continental psychologists in itself a convincing vindication of its programme? Does the 'anthropological turn' really mark an epoch? Or shall we say that these psychologists share in a provincialism<sup>17</sup> which may also be the hallmark of PA? Has it perhaps, in Hume's words, despite all its suggestiveness, fallen into 'that error, into which many have fallen, of imposing their conjectures and hypotheses on the world for certain principles'?<sup>18</sup>

## V

The question naturally arises why British philosophers should acquaint themselves with Continental philosophical pursuits such as PA. Their instinctive reaction is likely to be one of spurning studies which appear not sufficiently critical to be taken seriously by the philosopher, however interesting some of them may be from a religious or anthropological or metaphysical point of view.

<sup>17</sup> So P. R. Hofstatter, *Psychologie*, Fischer Lexikon 6, 1957, p. 9. Regarding Binswanger's existential analysis R. de Rosa speaks of a confused admixture of theoretical, empirical, and philosophical elements. 'Existenzphilosophische Richtungen der Psychopathologie', in *Offener Horizont*, Festschrift für Karl Jaspers, 1953, p. 189. However, Binswanger's existential analysis seems now to have carried the day among European psychotherapists. See F. E. Heinemann's report on the Fourth International Congress of Psychotherapy in September, 1958, *Philosophy*, XXXIV, 1959, pp. 356-357.

<sup>18</sup> This point has been forcefully argued by J. Kraft, *Von Husserl to Heidegger*, pp. 145-146.

However, this situation, far from being novel, falls into a well-established pattern, namely the traditional *bellum interneicum* between the schools which J. S. Mill gave its classical expression in his essays on Bentham and Coleridge. The mutual imputation of intellectual and moral obliquity, of sensualism and mere empiricism, on the one hand, and of mysticism on the other, has been a self-perpetuating quality of this debate. 'They seem to have scarcely a principle or a premise in common. Each of them sees scarcely anything but what the other does not see'. They blink 'the importance, in the present imperfect state of mental and social science, of antagonistic modes of thought . . .'<sup>19</sup>

The large and vast generalities pervading Continental philosophy invite British philosophers to use Occam's razor. Continental philosophers, too, mean to use this tool, though it is apt to be as blunt in the process of phenomenological reduction as it is apt to become the hangman's sword in the hands of many British philosophers. It is, I believe, true that Continental philosophy has often failed in its task to employ an adequate degree of clarity and critical doubt and thus to contribute to a sober general climate of opinion. However, there are sufficient pragmatic reasons for taking notice of the currents from beyond the Channel. I can here only hint at them.

1. Present-day currents determine or, at least, indicate the likely trends of future thought and action, and as ideas, they are apt to penetrate into other systems of thought *some* time. This, for instance, has happened in the cases of Continental sociology and of jurisprudence at a very belated date.<sup>20</sup> If Durkheim, Max Weber, or Simmel are of some importance today in Anglo-American thought, then, no doubt, it will be relevant to watch the development of their thought in their native soil. Unless this is done openly and thoroughly it is certain to be done piecemeal and in disconnected fashion, irrespective of consciously admitted likings and dislikes. It appears to be better to keep abreast of such developments and deal with them on their actual merits and demerits.

2. Metaphysics and ontology are widely regarded as obsolete by British philosophers. But their Continental colleagues discount such statements. Bochenski, for instance, maintains that there are a primitive ontology and dogmatic presuppositions underlying con-

<sup>19</sup> *Dissertations and Discussions*, 3rd ed., pp. 395, 399, and *passim*.

<sup>20</sup> See Noel Annan, *The Curious Strength of Positivism in English Political Thought*, Hobhouse Lecture No. 28, 1959. Annan's verdict, however, is far too sweeping. A plea for taking into account Max Weber does not entail that his work was of greater importance than, say, that of the Webbs. Another striking example is T. E. Hulme's belated presentation of the thought of Riegl and Worringer.

temporary British philosophy. The same criticism is made by Myrdal, and it has recently, at least in part, been supported by Iris Murdoch and J. O. Wisdom.<sup>21</sup> Continental critics of Hume from Kant to Husserl and to Deleuze have consistently pointed out an alleged metaphysical basis underlying Hume's thought. The excesses of Continental metaphysics and ontology are not excused by these counter-charges. However, if it is true, as I believe it to be, that we cannot escape from making metaphysical assumptions, then there is some merit in facing the issue rather than treating such questions as taboo or accepting, without questioning, the presuppositions of scientists. Again, the issue is a traditional one. 'There is no logic to the principles of which we can here appeal. There are different criteria of reality, and what is the reality of one is the mere phenomenon or illusion of the other'.<sup>22</sup> Another counter-charge on the part of Continental philosophers concerns the alleged fact that contemporary British philosophers, too, though paying lip-service to their respect for the results of science, actually take liberties with such results; that, for example, they pay scant attention to relevant psychological insights and put their own 'commonsense' interpretations in the place of scientific theories.

3. The dissimilarities between British and Continental philosophies are obvious. It is, however, possible to observe parallel developments in some respects. This is not so surprising in matters of methodology, which in itself is neutral, such as the development of mathematical logic. This approach is more widespread in the English-speaking world, though certainly not neglected on the Continent, where much of it was developed. However, there is probably more scepticism about the limits within which the application of 'heterodox' systems is permissible. Bochenski, for instance, insists on the necessity to use other methods besides the purely formalistic approach, which he wishes to see restricted to operative, abstract systems.<sup>23</sup> Altogether, advanced thinkers within the main stream, such as Bochenski, tend to apply the devices of modern methodology, making use of the investigations of Carnap, Reichenbach, Popper, etc. On the other

<sup>21</sup> I. M. Bochenski, *Europäische Philosophie der Gegenwart*, 2nd ed., 1951, p. 83; Gunnar Myrdal, *The Political Element in the Development of Economic Theory*, trsl. 1953, *passim*; J. O. Wisdom, 'Esotericism', *Philosophy* XXXIV (1959), p. 341; Iris Murdoch, 'A House of Theory', in *Conviction*, ed. Norman Mackenzie, 1958, p. 227. A perceptive treatment of 'the positive picture of the world' is contained in chap. IV of P. Munz, *Problems of Religious Knowledge*, 1959.

<sup>22</sup> J. Grote, *Exploratio Philosophica*, II, p. 324.

<sup>23</sup> See also C. Lejewski's 'On Lesniewski's Ontology' in *Ratio* I, 1958, pp. 150-151, for an account of Lukasiewicz's and Lesniewski's attitude towards 'mathematical games' and 'pure formalism'.

hand, a large number of philosophers believe their appropriate preoccupation to be something basically opposed to science; they accuse each other of 'atomistic' or 'mechanical' or 'abstract' thinking as often and as mutually as, for instance, British philosophers have, in another context, applied the device of the 'naturalistic fallacy'. Yet again, more thoughtful philosophers such as Dilthey, Jaspers, and Binswanger favour a complementary use of the methods of natural science and of phenomenology or similar methods.

Furthermore, it is worth pondering how far the 'spirit of the age' finds expression irrespective of different lines of approach. It is intriguing to compare the work of a leading philosophical anthropologist such as the Swiss zoologist Portmann<sup>24</sup> with that of Gilbert Ryle. Their working canons are distinctly different. Yet their aims are closely related to each other. Both reject the methodological division between the natural and the social sciences. Both aim at overcoming the Cartesian separation of body and mind. Both wish to expose the misleading 'anthropologies' underlying the approaches of the different sciences such as biology, psychology, sociology, ethnology, etc. Both therefore develop some sort of psychology of their own, Portmann in the hermeneutic tradition of 'geisteswissenschaftliche' psychology of Bergson and Dilthey, Ryle by means of linguistic analysis. For Portmann the subject-matter of thought is man as a mysterious unit. For Ryle man is not to be explained in a metaphysical or mechanical fashion; yet, 'there has yet to be ventured the hazardous leap to the hypothesis that perhaps he is a man'.<sup>25</sup>

There is another direction in which similarities in trends may be observed. The climate of opinion in British political philosophy has for some time been anti-theoretical both in liberals such as Weldon and in conservatives such as Oakeshott. This has led to a relativist and conformist acquiescence in the 'status quo'. The same attitude, in the wake of the romantic Historical Schools, has been widespread on the Continent where lawyers, historians, and philosophical anthropologists (though not Scheler amongst those treated here) have excelled in supporting the 'status quo'. However, on the British side the case of the 'good life' has gone by default—ostensibly, as not presenting an appropriate subject-matter for philosophy and, practically, because most people have come to accept the rational solutions of the utilitarian tradition.

<sup>24</sup> A. Portmann, 'Um eine basale Anthropologie', *Wirtschaft und Kultursystem*, 1955, pp. 286-300; *Zoologie und das neue Bild des Menschen*, 1956, with further bibliography pp. 137-138.

<sup>25</sup> *The Concept of Mind*, p. 328.

Continental philosophers, on the other hand, have felt tempted actively to glorify irrational choices.

4. Though there are similarities they are, however, inadequate for bridging the gap between the respective traditions. If I may try to point to what may be the essential difference between these traditions, it appears to me that modern British philosophers have predominantly concentrated on the refinement of methods and rules while Continentals have attempted to wrestle with substantive assumptions. British philosophers are therefore appalled by the poverty of critical acumen in some representative Continental thought. And Continental philosophers are puzzled by what appears to them the lack of commitment to vital ideas and the triviality of subject-matter in British philosophy. Both sides feel that criticism from the other end does not strike at the relevant elements of their thought. At the same time, it becomes clear where they may complement each other. Continental philosophers are in need of British methodological clarity and exactness. But British philosophers can do with Continental eagerness in asking questions and with some scepticism regarding the border-line beyond which clarity is bought at the price of the exclusion of meaningful and relevant questions. As a matter of fact, there is a discernible trend in recent British philosophy towards a renewed stronger interest in the 'great issues' of the human condition, away from the fascination with the technicalities of analysis or with the material world.<sup>26</sup>

It is comparatively easy to arrive at agreement regarding techniques and rules. It is more difficult to reach understanding on what is substantially relevant and 'important'. It is in this respect that PA has something of interest to offer, namely its variety of imaginative and possibly fertile visions and interpretations. However, its adherents fail as a result of a frequently insufficient submission of their theories to severe criticism and to severe tests. This weakness provides good reason for rejecting much of their thought and for a sceptical scrutiny of all their propositions in the context of their problem-situations. Yet this is no reason to by-pass *in toto* their tentative contributions to such questions as the autonomy of ethics, the philosophy of history and of religion, and the relationship of philosophy and myth. There is,

<sup>26</sup> See J. A. Passmore's review of Warnock's *English Philosophy since 1900* in this *Journal* 37, 1959, pp. 79-81. P. F. Strawson's *Individuals* and Stuart Hampshire's *Thought and Action* are cases in point. Popper's philosophy may serve as an example of a judicious fusion of what is best in the two traditions. The same view could be expressed regarding J. S. Mill's philosophy. The influence of earlier British thought on Continental philosophy and sociology is generally underestimated, largely because of the lack of sympathy shown by many contemporary British philosophers for some significant aspects in the thought of their predecessors.

however, one important point to make in this context. It is not quite fair to contrast Continental philosophy with British philosophy as such. As Continental philosophers cast their nets more widely a comprehensive comparison must take note of Anglo-American anthropology, psychology, and generally of the behavioural sciences. If this is done it will be found that most of what Continental philosophy may have to offer has been absorbed and, indeed, put to more critical use. It may be said that British philosophy has erred by excluding, and Continental philosophy by usurping the subject-matter of the sciences. If this points a moral it is that the very idea of specialisation requires to come under review. Though English-speaking philosophers have been impervious to Continental irrationality they may have been too credulous in emulating the image of the traditional German method- and system-ridden scientist, the successor of Browning's grammarian.

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# ARE METAETHICAL THEORIES NORMATIVELY NEUTRAL?

By WILLIAM T. BLACKSTONE

A. J. Ayer has argued that "all moral theories, intuitionist, naturalistic, objectivist, emotive and the rest, in so far as they are philosophical theories, are neutral as regards actual conduct".<sup>1</sup> These analyses of moral concepts or metaethical theories do not dictate or recommend certain kinds of conduct. They entail no normative ethical judgments. Metaethical theories are only analyses of "what people are doing when they make moral judgments".

Since the time of Ayer's statement, a number of philosophers<sup>2</sup> have become concerned with the relation between metaethics and the moral life or the relation between analyses of the meaning of moral concepts and normative ethical judgments. They have asked the question: "Are metaethical theories normatively neutral?" This question has been answered differently by different philosophers. In fact, the question itself has been interpreted as asking different things. The purpose of this paper is to set forth six fundamentally different interpretations of the question, "Are metaethical theories normatively neutral?". We will also propose answers to each of these formulations of the question. It is hoped that this procedure will clarify the relationship between metaethics and normative ethics.

## I

The question, "Are metaethical theories normatively neutral?", has been interpreted as "Do metaethical theories affect one's moral life?". Olafson,<sup>3</sup> for example, argues that the acceptance of a metaethical theory does affect our first-order moral life and that this effect is observable in a modification of our procedures of moral judgment. He is primarily concerned with the emotive theory and it is his specific contention that the acceptance of the emotive theory changes our conception of what we are doing when we make moral judgments, and further that it changes our view of the justification of such judgments. The emotive

<sup>1</sup> A. J. Ayer, *Philosophical Essays*, p. 246.

<sup>2</sup> For example, see Frederick Olafson, "Metaethics and the Moral Life", *Philosophical Review*, vol. 65, 1956; Paul Taylor, "The Normative Function of Metaethics", *Philosophical Review*, vol. 67, no. 1, January, 1958; and S. A. Grave, "Are the Analyses of Moral Concepts Morally Neutral?", *The Journal of Philosophy*, vol. 55, no. 11, May 22, 1958.

<sup>3</sup> Frederick Olafson, *op. cit.*

theory, as a metaethical theory, forces us to recognize the primacy of emotion in morals. This "performatory analysis of moral judgments generates a performatory moral life".<sup>4</sup> Acceptance of the metaethic of emotivism causes psychological or attitudinal changes in one. It makes one feel that rational principles are not operative for morals and that one's moral judgments are non-logical acts of preference.

Olafson, I think, is correct in his contention that acceptance of the emotive theory causally affects one's attitudes in morals. Although the emotive theory need not entail nihilism or chaos in ethics,<sup>5</sup> there is good evidence that acceptance of the view that moral judgments are mere expressions of emotion causes one to adopt a different attitude toward justification in ethics, and may in fact cause one to make different normative ethical judgments from those made prior to the acceptance of the metaethic.<sup>6</sup> It seems to me that the acceptance of any theory involving meta-linguistic analysis results in such causal effects. Certainly the meta-linguistic statement that religious or theological statements are non-cognitive has a causal effect upon one who operates within a religious framework. The belief that religious utterances can be neither true nor false has very serious causal effects when entertained by one who has religious beliefs, for such a meta-linguistic statement is even more devastating than being told that one's religious beliefs are false. It surely makes one feel as if no rational principles are operative in religion and that one's religious judgments are non-logical acts of preference.

Our point is that philosophical analysis of any area of discourse has a number of practical causal effects and this certainly includes analyses of moral concepts. It is certainly possible and in fact highly probable (though not logically necessary) that acceptance of a given metaethic, whether it be the emotive theory, the subjectivist theory, or the view of the intuitionist, will have a tendency to modify one's behaviour. It may (though it need not) result in one's making different normative ethical judgments from those made prior to the acceptance of the metaethic. Thus our answer to the first formulation of our question, namely, "Do metaethical theories affect one's moral life?", is a probable Yes.

## II

The question, "Are metaethical theories normatively

<sup>4</sup> *Ibid.*, p. 175.

<sup>5</sup> See W. T. Blackstone, "Objective Emotivism", *The Journal of Philosophy*, vol. 55, no. 24, November 20, 1958.

<sup>6</sup> See Paul Edwards, *The Logic of Moral Discourse*, p. 240, for an analysis of the consequences of adopting the emotivist metaethic.

neutral?", can also be put in this manner: "Do one's normative ethical beliefs logically entail one's metaethical theory?".

A look at the history of ethical theory will convince one that the normative ethical views of some philosophers certainly did dictate their metaethical theories. Let's take one such example. Bentham makes the following statement: "Of an action that is conformable to the principle of utility, one may always say either that it is one that ought to be done, or at least that it is not one that ought not to be done. . . . When thus interpreted the words *ought* and *right* and *wrong*, and others of that stamp, have a meaning: When otherwise, they have none".

Bentham's commitment to the principle of utility has led him here to assert the metaethical thesis that the normative terms "ought", "right", etc., could mean nothing other than that specified by the utilitarian. His thesis here is one about the meaning of ethical terms, not a thesis about what we ought to do. But it is clear that his position in normative ethics—that we should act so as to increase the happiness and welfare of all people—is the foundation or source for his metaethical thesis. Bentham's metaethical analysis of the meaning of "right" or "ought" is a reflection of his own ethical evaluations, and his preclusion of other metaethical theories is a logical consequence of his acceptance of the normative ethical views of utilitarianism. He has so defined the "ethical" that an act *must* conform to the principle of utility to be an ethical act. This makes his metaethics a logical entailment of his normative ethics.

It is no doubt true that the normative ethics of many other historical ethical theorists have dictated their metaethical views. However, a metaethical theory need not be a reflection of one's ethical evaluations. One need not arbitrarily define ethical concepts in terms of one's own normative ethical commitments such that one's normative ethics logically entail one's metaethics. That is, one's metaethics can be morally neutral even though it is a historical fact that many metaethical positions are not. The "ethical" or terms like "ought", "right", etc., could be defined in terms of use and function—in a morally neutral manner. One could view "ethics" as any system of norms for human conduct—a system of rules which function as regulative devices. Viewed in this manner, the subject matter of metaethical analysis would include the codes of behaviour advocated by anyone—those of Nietzsche, Kant, Mill, St. Paul, Hitler, etc. A metaethic of this type would be a theory about the meaning of all distinctly ethical terms and statements (any term or statement which functions in this regulative sense) and this metaethic would not be a logical entailment of any given normative ethics or a reflection of one's

own ethical evaluations. A metaethic of this morally neutral type would be a result of an analysis of the features and functions of discourse in which terms and statements function in a normative, regulative sense concerning human conduct. A metaethicist may, for example, conclude from his analysis that all terms and statements which function in a normative, regulative sense are non-cognitive expressions of emotion (the metaethic of emotivism). This conclusion would apply equally to the normative ethical principles of Nietzsche, Kant, Mill, St. Paul, Hitler, etc., and would not be a logical entailment of any given normative ethics. Our answer, then, to the second formulation of our question, namely, "Do one's normative ethical beliefs logically entail one's metaethical theory?", is that although this has been true of many historical ethicists, one's metaethics can be morally neutral.

### III

The question, "Are metaethical theories normatively neutral?", can be and has been interpreted in this manner: "Do metaethical theories entail certain normative ethical statements or moral claims?". For instance, is it the case that the metaethical view that moral judgments are non-cognitive expressions of emotion entails any particular normative ethical judgments? Or does the metaethic of subjectivism, the view that moral judgments are statements of individual approval or disapproval, entail any particular normative ethical judgments? Ayer's answer to this question is negative and I agree with him. Any given metaethic, including the emotive theory, aims at showing people what they are doing when they make moral judgments—not at suggesting which moral judgments they are to make. Furthermore, without logical inconsistency, one can be for or against the same sort of actions regardless of whether one adopts the metaethic of the emotivist, the subjectivist, objectivist, or intuitionist. One is not logically committed to capital punishment, trial marriage, or euthanasia—no matter what metaethic one adheres to. This is to say that there is no formal incompatibility between any normative ethical statement and any metaethical theory. This conclusion, however, requires that one's metaethical theory be morally neutral in the manner specified above, namely, that it not be a reflection of one's own normative ethical position. It should be noted, however, that the acceptance of any given metaethic may *cause* or influence one to accept a certain moral position. Our answer, then, to the third formulation of our question, "Do metaethical theories logically entail certain normative ethical statements?", is negative.

## IV

A fourth formulation of the question, "Are metaethical theories normatively neutral?", is "Do metaethical theories logically entail certain accounts of moral justification?". Metaethical theories are theories about the meaning of ethical terms and ethical statements, and, it seems to me, any given theory about the meaning of ethical terms and statements does logically entail a particular account of moral justification. The metaethic of subjectivism, for example, which views the meaning of ethical statements as autobiographical statements of approval or disapproval, entails a particular account of moral justification, namely, that the only data relevant to the justification of moral judgments are the autobiographical facts of personal approval or disapproval. The metaethic of emotivism, which views ethical judgments as non-cognitive expressions of emotion, certainly logically entails that moral judgments cannot be justified as true or false since moral judgments do not *assert* anything. For those who adopt the metaethic of emotivism, if there is disagreement in moral attitude between two persons who agree on the facts of the case, there is no method of resolution besides persuasion or abuse. On the emotivist's scheme, all the reasons relevant for a *purely* moral disagreement (as opposed to a disagreement in belief) are persuasive reasons, for, as Ayer puts it, moral principles have no "objective validity". Thus one analytic entailment of the emotive metaethic is that all ethically relevant reasons are persuasive reasons. Without noting other metaethical theories, it is clear that the manner in which one is to justify moral judgments depends logically upon what those ethical judgments are interpreted as meaning. Our answer, then, to the fourth formulation of our question is affirmative.

## V

A fifth sense of the question, "Are metaethical theories normatively neutral?", is "Are metaethical theories set forth as descriptively true theories or as prescriptions concerning the way moral language ought to be used and interpreted?". (We have already discussed above how a metaethic may be prescriptively set forth in the sense of being a logical entailment of a given normative ethic. We are here concerned with a different manner in which a metaethic may be prescriptive.)

If metaethical theories are set forth as descriptively true theories, the question arises as to what it is that they describe. Quite often a metaethicist maintains that his theory is a descriptively accurate account of the way that moral language is used.

The emotivist, for example, often seems to make this appeal to the way that moral language is used. If the emotivist's metaethic is based on this appeal, then it seems that it is descriptively false, for moral discourse certainly employs all the devices characteristic of cognitive discourse. As Glassen<sup>7</sup> points out, moral judgments often take the form of indicative sentences just as do other sentences known to be cognitive. Moral judgments also often appear in indirect discourse as the object of a cognitional verb like "know" or "believe". One quite often finds judgments like "I know that x is the right thing to do". Furthermore, appraisal terms like "true", "false", "correct", and "mistaken", are often applied to moral judgments. Thus we find persons saying: "It is true that x is right". This linguistic evidence *prima facie* supports a cognitive interpretation of moral discourse, and hence the appeal to "use" does not support the emotivist's metaethic.

However, the emotivist also maintains that his analysis is the correct analysis of what moral concepts *really* mean and what these concepts *really* mean is quite different from what they are taken as meaning in ordinary moral discourse. Ethical expressions are *really* expressions of emotion, not statements which can be true or false. But what are we to make of the emotivist's contention that his view is the correct interpretation of the *real* meaning of ethical concepts? What he appears to be doing is setting forth his metaethic as a prescription of how we should interpret and use moral language. This means that his metaethic is not normatively neutral in one sense of "normative neutrality". His metaethic, requiring that we view moral language as emotive, is based on a norm which states what should be accepted as being cognitively meaningful. The phrase, "real meaning of ethical terms", involves a reference to this norm. In the case of at least some emotivists, that norm is the principle of empirical verifiability. The metaethic of intuitionism rests upon a different criterion of meaning from that of the emotivist, and hence the prescription of the intuitionist concerning the way that moral language ought to be used and interpreted differs from that of the emotivist. In neither the case of the intuitionist's metaethic nor the case of the emotivist's metaethic is an appeal made merely to a descriptive account of the way that moral language is used. Our answer, then, to the fifth formulation of our question, "Are metaethical theories normatively neutral?", is negative.

## VI

A sixth possible interpretation of the question, "Are meta-

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<sup>7</sup> See Peter Glassen, "The Cognitivity of Moral Judgments", *Mind*, vol. 67, no. 269, January, 1959; and Frederick Olafson, *op. cit.*

ethical theories normatively neutral?", is "Does metaethical analysis have a normative function?". Paul Taylor<sup>8</sup> has argued for an affirmative answer to this question and I think that he is correct. The normative function of metaethics is to introduce greater rationality into our moral life. This function is fulfilled if it makes "our second-order beliefs about our first-order moral discourse more clear, coherent and true. If a metaethical analysis can correct our second-order errors and clear up our second-order vagueness and confusion about the nature and logic of our moral experience, then it can in turn help us to be more rational in our first-order moral deliberations and judgments".<sup>9</sup> For Taylor the criterion for a valid metaethic is whether that theory has the capacity for making us more rational in our moral life. A metaethic has that capacity if it satisfies the following tests: (1) "Does it increase our factual understanding of what happens when, in the ongoing practical situations of everyday life, we deliberate about moral issues, arrive at moral decisions, make and justify and argue about moral judgments, and in general carry on moral discourse with others?" (2) "Does it provide tools for analyzing moral discourse in such a way that we are better able to recognize the presence of ambiguity and vagueness in moral language, and to identify the ways in which, and the purposes for which, such language is used?" (3) "Does it enable us to carry on first-order moral discourse more clearly and intelligently?" (4) "Does it make explicit the rules of valid reasoning which are appropriate to moral argument; and does it make clear in what respects these rules are similar to, and in what respects they differ from, the rules of valid reasoning appropriate to matters of fact and to logically necessary propositions?" (5) "And finally, does it show us clearly how these rules can be applied in the everyday situations in which moral problems arise?"<sup>10</sup>

It seems to me that tests (2), (3), (4), and (5) are all heavily dependent upon one's answer to (1). If we are to be better able to recognize the presence of ambiguity and vagueness in moral language (2), we must have a clear understanding of what really goes on in moral discourse. Carrying on first-order moral discourse more clearly and intelligently (3) also requires an answer to (1), namely, a view of what really goes on in moral discourse. Furthermore, knowing what could be meant by "rules of valid reasoning appropriate to moral argument" (3) presupposes an answer to (1). If what really occurs in moral discourse is the expressing of emotion, then the meaning of "valid

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<sup>8</sup> Paul Taylor, *op. cit.*

<sup>9</sup> *Ibid.*, p. 28.

<sup>10</sup> *Ibid.*, p. 29.

reasoning" in ethics is radically affected. Finally the application of rules of valid reasoning to moral problems (5) requires an answer to (4) which in turn requires an answer to (1). Taylor's proposed test for the acceptability of a metaethical theory, then, seems to boil down to whether the theory "increases our factual understanding of what happens when, in the ongoing practical situations of everyday life, we deliberate about moral issues, arrive at moral decisions, make and justify and argue about moral judgments, and in general carry on moral discourse with others".

It appears to me to be analytically true that if a metaethical analysis provides this "factual understanding", then it fulfils the normative function of providing the *capacity* for making us more rational in our moral life (though not necessarily actually making us more rational). But is it not the case that all metaethical analyses are set forth as "factual understandings" of what really happens in moral discourse? How do we know which metaethic fulfils this normative function? In other words, how do we determine what constitutes a "factual understanding" of moral discourse? This question can also be formulated in this manner: What is the test for the "real meaning" of ethical terms and ethical statements?

Would a descriptive account of the way moral language is used tell us what really happens in moral discourse or give us the *real meaning* of ethical terms? Taylor answers this question affirmatively. Speaking of moral concepts, he states: "A metaethic simply tries to get us to be fully aware of what this use is and to know what to do when others challenge our use of moral expressions or when we become doubtful about our own use of them".<sup>11</sup> In becoming aware of the use of moral concepts, one becomes aware of their meaning. A valid metaethic, then, is apparently for Taylor one that correctly describes the way that moral concepts are used for the rôles that they perform in language.

Taylor's implicit criterion for judging whether a given metaethic constitutes a "factual understanding" of moral discourse seems to be based upon the implicit acceptance of the identification of meaning with use. We can discover what moral concepts really mean by examining their use or the rôles that they perform in language. If this criterion is employed, then it would seem that each of the metaethical theories—subjectivism, objectivism, emotivism, and intuitionism—is partially correct. Moral concepts and moral language have many functions or rôles. Moral language often performs the function of expressing one's emotion (the function stressed by emotivism). It quite often performs the

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<sup>11</sup> *Ibid.*, p. 31.

function of conveying autobiographical information (the function stressed by subjectivism). It also is quite often used to make assertions which are not autobiographical (the function stressed by the various forms of objectivism). Thus if we accept the Wittgensteinian identification of meaning with the use or rôles that expressions play in language, it may well be that intuitionism, objectivism, subjectivism, and emotivism are each correct in describing one rôle or function of moral language but each is incorrect in viewing that rôle as the exclusive rôle or function of moral language. On this procedure, a correct metaethic would be one that displays all the uses or rôles performed by moral concepts. A metaethic of this type will be more likely to provide clear, coherent, and true second-order beliefs about our first-order moral discourse, and help us to be more rational in our first-order moral deliberations.

The point I wish to make is that any answer to the question of what constitutes a "factual understanding" of moral discourse is logically related to the general theory of meaning that one accepts. Without arguing the point, it seems to me that the metaethic of emotivism is often logically related to the acceptance of the positivist's theory of meaning. It is also clear that the implicit account of meaning in Platonism produces a quite different metaethic from that which results from the acceptance of the positivist's theory of meaning, that which results from the position which identifies meaning with denotation, or that which results from the acceptance of the Wittgensteinian account of meaning. It seems clear, then, that an answer to the question of whether a given metaethic fulfils the normative function of providing us with the capacity for becoming more rational in our moral life (by providing us with a "factual understanding" of moral discourse) ultimately requires reference to a general theory of meaning. Since this is the case, then it seems to me that metaethicists should pay a great deal more attention to the problem of meaning than they have in the past and to the particular problem of criteria of adequacy for a valid theory of meaning.

In summary, we have examined six different interpretations of the question, "Are metaethical theories normatively neutral?". These interpretations of this question were: (1) Do metaethical analyses affect one's moral life? (2) Are metaethical theories logical entailments of normative ethical positions? (3) Do metaethical theories logically entail certain normative ethical statements? (4) Do metaethical theories logically entail certain accounts of moral justification? (5) Are metaethical theories set forth simply and solely as descriptively true theories rather than as prescriptions concerning the way moral language ought to be

used and interpreted? (6) Do metaethical theories have a normative function? Our answers to (1), (4), and (6) were affirmative, while our answers to (3) and (5) were negative. Our answer to (2) was affirmative for some historical cases, but we insisted that a metaethical theory need not be a logical entailment of a normative ethical position. Our discussion of (5) and (6), however, indicated that the acceptability or non-acceptability of a metaethical theory is closely related to the acceptability or non-acceptability of a general theory of meaning. The problem, then, of justifying a general theory of meaning is a paramount task for any metaethicist, for to speak of a "valid metaethic" requires that one speak of a valid theory of meaning.

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## ON THE ARGUMENT OF THE PARADIGM CASE\*

By ROBERT J. RICHMAN

Much has been said of late about the argument of the Paradigm Case,<sup>1</sup> which I shall refer to for brevity as the APC—although I do not think highly of it as a philosophical analgesic. That this argument has been so widely discussed is due in large measure, I suspect, to the fact that “Oxford” philosophers rarely make explicit the general principles on the basis of which they carry out their (frequently brilliant) philosophical inquiries, with the result that when one of their number does attempt to set forth one of these principles, his effort is likely to arouse a great deal of attention. Trusting that adverse criticism will not discourage further attempts at explication of principles, I shall argue (a) that the APC is not a conclusive argument and (b) that it is of little *philosophical* importance.

### I

Unfortunately, Urmson does not explain the APC with complete clarity. He does, however, explain its function:

“By it the philosophical doubt whether something is really an X is exposed as being in some way improper or absurd by means of a demonstration that the thing in question is a standard case by reference to which the expression ‘X’ has to be understood, or a doubt whether anything is X is exposed by showing that certain things are standard cases of what the term in question is designed to describe.”<sup>2</sup>

He also sets forth examples designed to clarify the nature of the APC.

“Suppose that someone looking at what we would regard normally as a typically red object expressed a doubt whether it was really red. He might indeed express doubts whether it was really red because he thought that the light was unusual, or that his eyes were bad, or something of that sort. But suppose that he expresses doubt for none of these reasons but doubts whether the term ‘red’ can properly apply to this sort of thing. We would then be at a loss and probably ask him what on earth he meant by red if he was unwilling to call this red, or say that by ‘red’ we

\* Read at the Christmas meeting of the Pacific Division of the American Philosophical Association at the University of Oregon, 1958.

<sup>1</sup> Set forth in J. O. Urmson’s “Some Questions Concerning Validity”, reprinted in *Essays in Conceptual Analysis* (London, 1954). For discussion, see, e.g., *Analysis*, Vol. 18, Nos. 2, 4, 5, and 6.

<sup>2</sup> *Ibid.*, p. 120.

meant being of just some such colour as this—‘If we do not call *this* red then what would we?’ Thus using a simple form of the argument from standard examples, we can make him see that there is something absurd in his question, since there is no better way of showing what the word ‘red’ means than by pointing to things of this colour.”<sup>3</sup>

I have quoted this trivial example (Urmson’s description) at some length since I am trying to become clear about the nature of the APC, and because the last sentence of the quotation seems particularly useful in this attempt. The APC appears to be closely related to the doctrine of ostensive definition. Its nature may be indicated as follows: The meaning of some term ‘X’ is learned by being shown things  $t_1$ ,  $t_2$ , etc., which have the property X. These things,  $t_1$ ,  $t_2$ , etc., are then standard examples, or paradigm cases of X. Since this is so, it is absurd to question whether, say,  $t_1$  is an X, or whether anything is an X.

First, a word about the term ‘absurd’. If the APC is to have more than merely persuasive value, the ‘absurd’ of its conclusion must be a logical or quasi-logical predicate (meaning something like ‘self-contradictory’). ‘Absurd’, in other words, must not be merely a pejorative term indicative of our dislike or disbelief of the conclusion; otherwise, the APC is not an *argument*. We cannot dispose of opposing views by calling them foolish.

Why, then, might it not be absurd to assert with regard to some predicate ‘X’ such as is referred to in the APC that nothing (really) is X, or that some particular thing is not X? Why, in other words, is the APC less than conclusive? Clearly, if we learn the meaning of ‘X’ by being shown things which are X, and if we have learned what ‘X’ means, then there are things which are X. This follows from the meaning of ‘show’: we can’t *show* someone things which are X unless there *are* things which are X.

Phrasing the argument in terms of what words *mean* may suggest that the APC is a completely semantical or logical sort of argument. But this suggestion is quite misleading. The premises of the argument are not logical in character—rather, they are psychological. They have to do with the way in which the meanings of certain terms are said to be shown or learned, and there is no logical absurdity involved in supposing that they are false. Most radically, there is no logical absurdity involved in supposing that we don’t *learn* the meanings of any terms at all. It is conceivable that we might at birth, or through a process of maturation, understand the meanings of terms generally, or of some particular terms. On this supposition, it is obvious that

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<sup>3</sup> *Ibid.*, p. 121.

some or all terms which we understood might have no *denotata*. But this supposition, while not perhaps logically absurd, is, I suppose, believed nowadays by no one. I shall therefore disregard it, except to point out that it alone is sufficient to show that the conclusion of the APC is not proved by the argument.

Let us assume that we do learn the meaning of terms. This assumption is obviously insufficient to sustain the APC, since the learning process might in every case be unlike that indicated in the APC. I wish to emphasize the contingent character of the supposition that we learn the meaning of (some) terms by being shown instances to which it applies. (I am not asserting that this suggestion is false. Neither, incidentally, am I denying that some things are red, that my desk is solid, or that John Foster Dulles exercises free will. But I do not think that any of these assertions can be conclusively demonstrated by the APC: anyone who seriously doubts any of them will be unconvinced by the APC, and not illogically so.) There is involved a question of psychological causation, not to be resolved *a priori*. It *might* be the case that we are led to understand the meaning of 'red' by being shown round objects, green objects, or elephants, by intravenous injection, or by surgical operation. As a matter of fact, I don't suppose that our knowledge of physiological psychology is sufficient to rule out the last two methods as physically impossible. But let us admit that we know what 'red' means, and that we haven't in fact learned this in any of these ways. Recognizing that the case of a term like 'red' (i.e., any term whose meaning can presumably be shown *only* ostensively) is a kind of paradigm case of the applicability of the APC, what could a person say against the APC if he wanted to deny, say, that anything is red? (We shall assume that our straw man makes use of none of the possibilities which we have already rejected, e.g., that the meaning of 'red' is not learned, or that it is learned in some logically possible, but odd way.) We can get a clue as to what he could say from the manner in which he would presumably formulate the statement of his position: "Nothing is *really* red". Granted a doctrine of ostensive definition, granted that the meaning of some terms is taught by exhibiting examples, still all that is required to show the meaning of 'red' are cases of *apparent* redness. As long as we can learn the meaning of 'red' from things that appear red, it cannot be logically absurd to assert that we have learned the meaning of 'red' ostensively and to deny that red things exist. The sceptic's position here is rendered strong by the semantical fact (a prime stumbling-block, incidentally, to phenomenism) that for no characteristic  $\phi$  is it the case that 'x appears to be  $\phi$ ' is synonymous with or entails 'x is  $\phi$ '.

One more word about 'red' (and similar terms). Suppose that our hypothetical sceptic were to deny not that anything is red, but that something, *t*, which we took as a paradigm case of redness, is red—and that he expressed no doubts about the lighting conditions, his eyesight, or the like. The situation would clearly be a difficult one. For if *t* represents a paradigm case of redness then *a fortiori* it represents a case of redness, i.e., *t* is red. But conversely, if someone denies that *t* is an example of a red thing then, of course, he denies that it is a *standard* example or a paradigm case of a red thing. Thus, Urmson's assertion that we can make the sceptic see the absurdity of his position by applying the APC seems unduly sanguine. There is needed some support for the contention that *t* is a paradigm case. There is needed at least a way of telling in controverted cases that an example we are dealing with is a standard example. What is the criterion which we employ for determining that a given putative case of 'X' is a *paradigm* case? This is an aspect of the APC which surely requires clarification. If, as the quotation from Urmson on 'red' suggests, a paradigm case is literally one on the basis of which the meaning of a term can be shown, then on the basis of *this* conception a paradigm case of 'red' might not be an example of a red thing at all. To see this, consider what happens in showing or teaching the meaning of 'red'. The child—as far as I know the ostensive teaching of the meaning of 'red' occurs almost invariably in childhood—comes to understand this term in part by being shown red objects. So far, so good. But because of lack of precision, and also because of inattention on the part of the teacher, many of the things on the basis of which an understanding of the meaning of 'red' is acquired may not be red. (I ignore, of course, the fact that part of coming to understand 'red' consists in being told that certain objects are not red; in a sense these objects are part of the basis on which the meaning of 'red' is shown.) The first point is of greater interest for our present purposes. In teaching the meaning of a term we may make use of examples which may no longer serve as examples as the concept becomes more precise. This point is clearer in other cases, but even in the case of 'red' the meaning of the term may be shown in part by pointing to things of which we should say, after our concept of red has become rather more refined, "No, that's not quite red—it's rather a shade of pink". Thus, e.g., *t* might be a paradigm case of redness (in the sense in question) and not be red, but rather, say, pink. Presumably then *this* is not the criterion of a paradigm case intended (but what does Urmson mean by the curious phrase "showing what the word . . . means"?), and the question as to how to recognize a paradigm case remains open.

So far I have dealt with the application of the APC to terms like 'red' which presumably are definable only ostensively. These are the cases in which the cogency of the APC is greatest, and yet even here there are grounds for denying that the APC is conclusive. Bearing in mind that the objections raised apply *a fortiori* to other sorts of cases, let us drop this line of inquiry since, APC or no APC, there is little if any disagreement about the applicability of terms like 'red'—disagreement, that is, of the sort which the APC is designed to resolve.

Let us look briefly at another example of the use of APC which Urmson cites. This example has the advantage of being directed against someone, namely, the physicist Sir Arthur Eddington. Urmson writes:

"Eddington said in effect that desks were not really solid. Miss Stebbing, in her book *Philosophy and the Physicists*, used the argument from standard examples to show that this way of putting things involved illegitimate mystification; this she did by simply pointing out that if one asked what we ordinarily mean by *solid* we immediately realize that we mean something like 'of the consistency of such things as desks'."<sup>4</sup>

This last statement clearly won't do. What we ordinarily mean by 'solid' has no particular reference to desks;<sup>5</sup> we would be able to use the word quite intelligently if no furniture at all existed. Whatever the difficulties with the concept of analyticity, it is reasonably clear that 'this desk is solid' is not analytic. (One might be tempted to say that this is a paradigm case of a synthetic statement!) Yet, if 'solid' is synonymous with 'of the consistency of such things as desks', then 'this desk is solid' would be analytic.<sup>6</sup> (Note that different applications of the APC may lead to contradictory results.)

But it may be urged (a) that to say that this desk (e.g., Eddington's) is not solid is misleading and (b) that if we don't call this desk solid then "what would we?". As to (a) saying that the desk is not solid may be misleading because of an ambiguity in the term 'solid'. Among other things, 'solid' means (roughly) (1) 'without spaces or interstices' and (2) 'substantial, not flimsy'. It is clear from the context of Eddington's book that he is concerned with solidity in the former sense. If the assertion that the desk is not solid suggests, e.g., that the desk won't support such things as lamps and books, it is only because 'solid' is being taken in the second sense. Thus, Eddington's statement, read in context, *ought*

<sup>4</sup> *Ibid.*

<sup>5</sup> Indeed, Eddington writes not of desks but of tables.

<sup>6</sup> I assume that the use of 'this' obviates difficulties connected with considerations of existence. At any rate, 'all desks are solid' could serve as our example.

not to be misleading. (The suitability of a "realistic" interpretation of micro-physics is another question.) Moreover, as perhaps this example suggests, a statement can be misleading without being false (there are, of course, moral problems connected with *deliberately* misleading assertions), and almost any statement can be misleading to *some* individuals. As to (b) it might well turn out that there exists nothing which Eddington would call solid. This would show not that 'solid' has no meaning, but only that it has no denotation. If 'solid' means 'without interstices' it might happen that nothing satisfies this description; but we *would* call solid anything which did. (There is a question of the *usefulness* of this way of talking; I should suppose that this is a separate question from that of its absurdity.)

## II

Urmson's main point with regard to the APC in "Some Questions Concerning Validity" is not to defend it but to indicate a restriction on its applicability. I should like to argue quite briefly that if this restriction is necessary then (a) this is further evidence of lack of cogency of the APC and (b) the APC is rendered of little philosophical importance. The restriction on the use of the APC has to do with value terms: according to Urmson, the APC cannot legitimately be used to demonstrate the applicability of such terms since, in effect, to go from the factual premise that a value term is applied to certain things to the conclusion that the things in question are valuable is to be guilty of the Naturalistic Fallacy.<sup>7</sup>

Now if the APC were a valid type of argument this restriction would be unnecessary since it is clear that the meaning of some value terms is taught by means of examples. "How else *could* they be taught?"<sup>8</sup> If, then, this restriction is necessary, the APC is not a valid type of argument. The reason this restriction is said to be necessary is that in applying the APC to value terms, an implicit value premise is involved (and it is the task of the philosopher to make explicit and to consider critically this premise). But even with regard to descriptive terms the use of the APC involves a valuational leap, from 'X' is applied to t' to 'X' is correctly applied to t'. There is, moreover, a tacit factual premise involved in going from 't is called X' to 't is X'. For in order for people to call t an X (truthfully) they need only *believe* that t is an X, and in this belief they may be mistaken.

<sup>7</sup> See Antony Flew's approving comments on this point in "Philosophy and Language", *op. cit.*, pp. 19 f.

<sup>8</sup> Cf. Flew, *ibid.*, p. 19.

Even if there are cases in which the possibility of such a mistaken belief is at a minimum (the case of 'red', for example), such is not the case with philosophical terms. (I come at last to the irrelevance of the APC to philosophical questions.) I shall simply state that there is greater risk of error in the application of philosophical terms than there is in the application of 'red' (which fact is obvious enough anyway), since I should wish to assert that, generally, key philosophical terms are used evaluatively. Hence, on the view of the proponents of the APC themselves, the APC is inapplicable to them. Briefly: some philosophical terms are clearly evaluative, e.g., 'good', 'right', 'beautiful'. Others are seen to have evaluative force on reflection: 'valid' (Urmson's example), 'probable', 'meaningful', 'certain'. That is, the basic terms of ethics, aesthetics, logic, and epistemology are evaluative in character. (We may say crudely that in these disciplines we attempt to set forth criteria of what is good in the way of actions and character, works of art, arguments, and beliefs.) Metaphysics seems to be the exception. But even though it may be doubted that the key metaphysical terms such as 'existence', 'real', 'free will', etc., are evaluative, it is clear that they, like other "descriptive" terms, may be *used* evaluatively. I should suppose that one central reason for the failure to resolve metaphysical disputes is that historically they have been so used, that, e.g., at least from Plato on, metaphysicians have conceived of the real as the valuable. We find evidence of this valuational aspect of metaphysical terms in everyday speech wherein the derogatory 'mere' is applied to illusion and appearance, but not to reality, to automatic and habitual actions, but not to deliberate or free ones.

I wish by no means to assert that every philosophical term is used evaluatively. But when we are concerned with the analysis of a non-evaluative term (e.g., 'cause') we do well to see the relation of the term to ones which are evaluative (e.g. 'known', 'meaningful') if we wish to understand why the analysis remains long a topic of dispute, and why (among other reasons) the APC is in such cases of little use.

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## DISCUSSION

### THE ROLE OF CORROBORATION IN POPPER'S METHODOLOGY

By JOSEPH AGASSI

In his interesting "Critical Notice" on Popper's *Logic of Scientific Discovery* (this *Journal*, Vol. 38 (1959), No. 2, pp. 173-187), Mr. D. Stove discusses almost only one point, Popper's theory of corroboration. He finds in this theory two elements. The first is Popper's emphasis on the significance of the sincerity of the attempts to refute existing scientific hypotheses. This element Mr. Stove considers to be psychological and irrelevant to the problems at hand. The second element is Popper's view according to which a hypothesis is corroborated only when the (sincere) attempts to refute it have resulted in failure. This Mr. Stove considers a rather traditional solution to the problems at hand, and one which may be criticized by traditional arguments, as well as by new arguments which Popper himself states in his book.

I have used the phrase "the problems at hand" twice already; I could not be more specific as I am not quite certain what are the problems which Mr. Stove has in mind. I find it somewhat bewildering that Mr. Stove does not say more explicitly what problems Popper claims to be discussing, what problems Popper's theory of corroboration is intended to solve, or what problems Mr. Stove thinks this theory ought to solve. I wish, therefore, to supplement Mr. Stove's interesting but seriously limited review on these points as best I can. I shall explain why I think that his criticism is entirely valid, and that it is in no way a criticism of the book he was reviewing (except, perhaps, for a criticism of some ambiguity on Popper's part).

1. *Popper's problems and central tenets.* Popper claims to have solved two traditional philosophical problems: the problems of induction (how do we learn from experience?) and of demarcation between science and non-science (by what criterion do we decide which hypothesis is scientific?). His solutions are these: (1) We learn from experience by repeatedly positing explanatory hypotheses and refuting them experimentally, thus approximating the truth by stages. (2) Those hypotheses are scientific which are capable of being tested experimentally, where tests of a hypothesis are attempts to refute it.

(This is the core of Popper's book, as he himself claims,

and as, I should think, is rather obvious anyhow. In this core there is nothing explicit about corroboration, namely about the failure to refute hypotheses; but only of success in refuting some hypotheses, which success is alleged to constitute learning from experience. And Popper's criterion of demarcation does not distinguish between refuted and corroborated hypotheses. By Popper's criterion even hypotheses which were amply refuted by experience are fully entitled to the honorary status of scientific hypotheses.)

Mr. Stove's review relates almost entirely to Popper's theory of corroboration; he devotes two or three rather short paragraphs to the core of the book (pp. 173-174), without relating this core to the theory of corroboration in a clear and explicit manner. Moreover, in these two or three paragraphs he commits some significant errors of presentation.

For example, he attributes to Popper the view that "there is no problem at all about induction" although Popper stresses the opposite, namely that Hume's criticism, which establishes the invalidity of inductive inferences, leads to a few genuine philosophical problems, one of which is the problem of induction, namely, 'how do we learn from experience?'. Mr. Stove, apparently, translates 'how do we learn from experience?' into 'how do we know which of our hypotheses is true or at least probable?'. For Popper this traditional translation is a mistake. Mr. Stove, however, does not notice this, and having attributed the wrong question to Popper, he thereby attributes to him the wrong answer, to wit, the view that a well-corroborated theory is likely to be true.

2. *The traditional view of scientific "success".* Undoubtedly, the attitude towards Einstein's general theory of relativity changed dramatically with the result of Eddington's observation of the total eclipse, which agreed with the theory rather well. Such a result is viewed by physicists as a great "success", a "verification", a "proof", or "confirmation". Most physicists, I contend, display great pleasure when confronted with "verifications" and give them much publicity, while (with the important modern exception of the discovery of Lee and Yang) the refutation of hypotheses is displeasing to them and is toned down or even pooh-poohed.

If we accept Popper's view, then it seems that we have to view the scientists' traditional delight in "success" and dismissal of "failures" as their misunderstanding of their own activities, or rather as their method of advertisement which need not be taken seriously. And yet, Popper claims, there is an important element in "success".

3. *A descriptive theory of "success".* The empirical support which a theory gains from experience, or its "success", or "confirmation", or "verification", is a measure of its having stood up to severe tests, or, in Popper's terminology, its high degree of corroboration. This is the whole of Popper's doctrine of what "success" is. I wish to stress that the theory according to which "success" is failure to refute a hypothesis does not entail that one "success" leads to another. Nor does it entail that "success" is a Good Thing. Much less does it include the view, sometimes stated explicitly, and more often implicitly, that theoretical science is a Good Thing because it is the body of "successful" theories.

4. *Corroboration and eliminative induction.* Most men of science, and almost all philosophers, think that "success" is a Good Thing, while Popper thinks that "failure" is a Good Thing. Now, many people have stated before that the refutation of errors is a contribution to learning. But they usually agreed that this is only so because some people have preconceived ideas which are mistaken and which are obstacles to learning and therefore have to be refuted; they usually agreed that, had people been cautious and slow to advance hypotheses and to commit themselves to them, the drudgery of refutation could be greatly reduced. Some people, in particular the great though neglected philosopher, William Whewell, claimed that, as it is most unlikely that one should hit upon the true hypothesis before hitting upon false ones, refutation is a necessary preliminary to any discovery of a true law of nature. Yet even Whewell did not think that refutation is good in itself, but rather good as a preliminary which is a useful means for the discovery of the truth. This theory, that we discover the truth through the elimination of errors, is known as the theory of eliminative induction. In some versions of this theory, refuting errors does not necessarily lead to the truth, but, in any case, to hypotheses with high probability or high credibility. In Popper's terminology Whewell's theory can be put (in a somewhat improved fashion) thus: a well-corroborated theory is true, and its corroboration is its verification. The somewhat watered-down version, then, would be this: a corroborated theory is probable or credible.

This last doctrine has often been attributed to Popper.<sup>1</sup> And time and again he has claimed that this is not his view. In Popper's view no degree of corroboration of a hypothesis can secure that it will not be refuted in the next test; no degree of

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<sup>1</sup> See von Wright (*Logical Problem of Induction*, second edition, Oxford, 1957), H. G. Alexander (PJPS, Vol. 10 (1959), No. 39, p. 234) and S. F. Barker (*Induction and Hypothesis*, Cornell, 1957); this is by no means an exhaustive list.

corroboration of a hypothesis makes it even slightly more probable that it will not be refuted in the next test. Corroboration implies neither verification nor any increase of probability.

To put this somewhat differently, we can neither hope to escape error, nor to make error less likely—not even in a limited field of research. All we can hope is that we shall eliminate some errors, and replace them by other, smaller errors. This is the main point where Popper differs from the eliminationist inductivist. He views as ends what they view as means.

Strangely, Mr. Stove knows this point, but he cannot quite take it seriously. Dismissing this point as inessential to Popper's doctrine, he (rightly) finds Popper's doctrine to be essentially in the eliminationist inductivist tradition; but his dismissal of this point can be explained as his inability to grasp Popper's novel and revolutionary idea.

5. *On the so-called rational degree of belief.* The problem of induction—how do we learn from experience?—Popper has tried to solve *not* by his theory of corroboration but by his theory of gradual approximation to the truth by repeatedly making explanatory hypotheses and refuting them experimentally. Another problem is, how can we avoid teaching false views or at least diminish our liability to assert mistaken views? This question, says Popper, has only one answer: the less you say the less likely you are to err. But science is an attempt to say more and more about the world, so that those engaged in science should have no fear of asserting an erroneous view, but they should do the utmost to encourage criticism. The last traditionally important question is, what theory should we believe, and why? This question, which is central in the inductivist tradition, is hardly discussed by Popper. A glance at the index to his book will reveal how little he says about beliefs, and reading these passages on belief will reveal that most of them are critical of traditional views rather than constructive.

The faith in science is a faith in a certain open-mindedness and detachment of belief—even to the extent of toning down beliefs and viewing them as entirely private affairs. This is the opposite of the view that science tells us what are the true objects of belief, be those the true laws of nature or the most probable among the known hypotheses in the light of the known factual information.

In the beginning of his "Critical Notice" (p. 174) Mr. Stove correctly attributes to Popper the view that empirical

science can lead to disbelief, but not to belief. From the middle of his "Critical Notice" onwards, however, Mr. Stove identifies Popper's view with eliminative inductivism, whose major aim is to prescribe beliefs in probable hypotheses or in verified laws of nature. Take away from eliminative inductivism this prescription of beliefs, and you do get Popper's view. But then the problem arises, how do we gain theoretical knowledge from experience? The eliminative inductivist's answer to this question is, of course, that learning from experience is the same as finding which is the most probable hypothesis—the very answer which Popper rejects. His answer is that we learn from experience by refuting our hypotheses and inventing new explanatory hypotheses, and refuting these again, thus achieving better and better approximations to the truth.

6. *Knowledge and learning.* The problem of induction concerns the question of how we learn from experience, a problem which belongs to the theory of learning, or methodology. Traditionally, however, philosophers have mainly concentrated on the problem of whether or not we have knowledge based on experience, a problem which belongs to the theory of knowledge, or epistemology. There is a good reason for this: if we could show that we have knowledge based on experience, then we could thereby show that indeed we do learn from experience. But all attempts to show that we have knowledge based on experience have so far failed. One of Popper's major revolutionary approaches was to go back from epistemology to methodology. Popper takes for granted (as an empirical fact, if you will) that we do learn from experience, and he asks by which manner, in what way, we learn. It turns out that it is easier to discuss methodological problems than epistemological problems. This is why Popper has so little to say on epistemology though he says quite a lot on methodology.

I am afraid that Mr. Stove has completely missed the point. He claims (pp. 178, 180) that the difference between having invented a hypothesis to explain a set of facts and having discovered these facts as corroborations to that hypothesis is "just nil". In other words, he claims, no matter how we have arrived at our knowledge, it is the state of knowledge that matters. And he says further that discussing this question of how we arrived at our knowledge is a psychological matter. Thus, he claims in effect, there is room only for epistemology and for psychology, but not for methodology, which is outside these two fields.

Now the situation is this. It was Popper who strongly emphasized, in opposition to many inductivist philosophers (in-

cluding Bacon, Newton, and Mill), that from the point of view of assessing our present-day knowledge, the question of how we have arrived at that knowledge is entirely irrelevant. But though epistemologically irrelevant, this question is methodologically highly relevant. Here Popper argues not that it is irrelevant whether or not hypotheses are derived from facts, but rather that it is most often not the case that hypotheses are found by looking at facts; rather they are found by trying to solve concrete problems. This is not psychology. Similarly, though epistemologically it is irrelevant to ask whether we found facts by opening our eyes wide enough, methodologically it is most important to notice that opening one's eyes does not lead to the discovery of new facts, and that it is easier to arrive at new facts by trying to refute our present hypotheses. Psychologists may ask whether we like to refute our pet hypotheses; methodologically what matters is that we learn by doing so. Similarly, from the epistemological viewpoint there is no difference between having discovered facts before or after explaining them. This last assertion contradicts the central doctrine of Whewell. According to Whewell a hypothesis is verified only when it explains a new fact which it was not intended to explain. Now intentions and chronology are irrelevant to the question of whether the hypothesis at hand is true. And the fact that we are often more impressed with corroborations than with explanations is an irrelevant psychological fact. Now that we have agreed (Mr. Stove and myself) that corroboration is irrelevant to the appraisal of our state of knowledge, and that the fact that corroborations are impressive is a psychological irrelevancy, the question to be asked is whether corroboration constitutes progress of knowledge and, if so, why.

7. *Learning by corroboration.* The significance of corroborating evidence is twofold; first, it is in certain respects new evidence, and secondly, it illustrates the high explanatory power of the corroborated hypothesis. I shall not discuss the fact that high explanatory power is considered valuable, because Popper discusses this point at great length. I should only argue that the fact that we pay much attention to a corroborated hypothesis can better be explained by the desire for explanatory power than by the desire for credibility. A corroboration of a refuted hypothesis is pointless from the credibility viewpoint. And yet some corroborations to already refuted hypotheses were very important, because, I suggest, they increased their explanatory power. In this way they rendered it necessary to build future hypotheses in a manner which would yield the refuted but corroborated hypotheses as a first approximation. (This is Bohr's correspondence principle translated into Popper's system.) It is easy to say, after the event,

that the refuted hypothesis was important because it explained much, and not because it was corroborated; the fact remains that we learned that the hypothesis explained much by testing it further and by obtaining corroborations as the results of these tests.

Thus, we want to know not only whether a hypothesis is true or false, but also to find out, as sharply as possible, the limits of its explanatory powers, both from within (corroboration) and from without (refutation).

There is a third argument in praise of corroboration which I put diffidently because, although it is quite simple and may be of some importance, I find it difficult to construct an example for it. Imagine first a development in which a hypothesis *A* is refuted by the fact *a*, a hypothesis *B* which explains both *A* (as an approximation) and *a*, but is refuted in the first test by the fact *b*, and a hypothesis *C* which explains both *B* (as an approximation) and *b*. Imagine a second process, starting with the same hypothesis *A* and its refutation *a*, being followed by the hypothesis *C* which is later corroborated by the fact *b*. Here, not the corroboration, but the skipping of the stage *B*, is what makes the second process more rapid than the first. The corroboration is the result of the rapidity, and not *vice versa*.

To conclude, the value of corroboration lies in the discovery of the corroborating facts, in the discovery of as many facts as possible which are explicable by an existing theory, and in its being a result of rapid progress.

8. *Is corroboration really necessary?* So far I have argued that the corroboration of a theory is enlightening—though less enlightening than its refutation—even if it is not really necessary. Mr. Stove, rooted in his inductivist tradition, takes it for granted that factual knowledge which is acquired after inventing a hypothesis could have been acquired beforehand. This is clearly not so with refuting evidence, which is observed as a result of a long chain of deductions of a prediction from a hypothesis. But since, as Mr. Stove observes, the corroboration of a hypothesis is a refutation of a previous alternative to it, perhaps corroborating evidence could be observed before the invention of the hypothesis it corroborates. I do not wish to decide this problem, but merely to point out that while Mr. Stove knows the answer to it as a matter of course, I find it worthy of a critical discussion.

It is a famous idea, already used by Galileo against inductivism, that we see what we think we ought to see, that we interpret facts in the light of theories. This idea, that experience usually tells us only what we have already thought out for our-

selves, makes it particularly difficult to see how we can learn from experience. Both Galileo and Popper have claimed that we can escape this limitation (to some extent) by being extremely critically minded. But Popper, at least, admits that this is easier said than done, and even when we are on the alert we often tend to see things as we expect them to be.

Because of this, Popper suggests, it is preferable to have a number of alternative hypotheses, and to design crucial experiments between them. In this case one hypothesis may be a useful instrument with which to refute another, and in the process of using it we may corroborate it.

There exist striking historical examples to this effect. The deviation of Mercury from the path prescribed to it by Newton's mechanics was not viewed as a refutation of that hypothesis. After all, a few apparent deviations had occurred before, and these were later satisfactorily accounted for without involving overthrow of that hypothesis. Moreover, only after Einstein had explained Mercury's deviation along new lines was a similar, though smaller, deviation discovered in other planets, including Earth, in accord with Einstein's hypothesis. A debate is going on at present as to whether the other two corroborations of Einstein's hypothesis are not in fact refutations of it; I suggest that an alternative to Einstein's hypothesis which would be in better agreement with these facts may alter the widely accepted attitude towards them (especially if that alternative be corroborated). So at least in some instances one may doubt Mr. Stove's assumption that corroborating evidence could be discovered prior to the hypothesis it corroborates, at least in the sense that the evidence is viewed entirely differently when it is a corroboration, in the sense that what might be considered as small deviations or even statistical errors are now viewed as important facts.

But, we should notice, there exist important refutations which were never considered as corroborations. The Michelson-Morley experiment and the Lee and Yang experiments are famous examples of this.

It is obvious, since essentially we cannot predict the result of a test, that we cannot have a satisfactory explanation of corroborations in general. Nevertheless, I wish to express my profound sense of puzzlement at the incredible corroborations which some theories have received, in the past and in our own times. I cannot escape the feeling that it is as if a deity paid us a premium for any good explanation and let it be well corroborated before it be refuted. To use the inductivist language, I am willing to bet at any odds that if a reasonably good solution should be found to any of the major problems in contemporary physics, it

would be well corroborated within a short time, and with much greater ease than it would be later refuted. I cannot support this feeling, which, of course, may be totally pointless. And I do not think that Popper's philosophy so far explains the amazing fact, if it is a fact, that so much corroboration is to be found in the history of science, although, I think, Popper's view is the only reasonable explanation of the value of corroboration.

But, I should add here, in a recent public lecture<sup>2</sup> Popper has claimed that corroboration is important, apart from being enlightening as discovery and increasing the explanatory power of a hypothesis, and is even essential to science—as encouragement to our research. I confess that I do not quite see this point, and I would here join Mr. Stove in asking what is the essential difference between corroboration and explanation. If psychological factors are ignored, and if the corroborating facts would have been found before the hypothesis which they corroborate, why, then, is corroboration essential?

9. *The indispensability of the corroboration of factual reports.* As Popper has pointed out, one kind of corroboration is essential for science. It is the corroboration of factual evidence, or rather of its spatio-temporal universalization, which is often called "a general fact" or "an observable" or "a generalization". We know that only repeatable facts are considered in science, and repeatable in the sense of corroborable, not in the sense in which the reports about flying saucers are constantly flowing in. The reason for this is that a fact is important when it contradicts a theory. And it is important to have them corroborated in order to make it simpler to accept the reports as those of general facts rather than to explain them away in some *ad hoc* fashion.

Popper has no wish to explain the fact that the generalizations we propose are often corroborated: it is the task of specific scientific hypotheses to explain specific generalizations which are corroborated (and thereby to explain the fact that they are corroborated). This would not satisfy an inductivist, since such specific explanations are dubious; he wants certitude or at least high probability. But he is asking for the impossible. All one can say in a general manner is that if we had had no corroborable general facts we would not have science as we know it. But a world with no general facts is, perhaps, one in which even life is impossible.

10. *Conclusion.* Mr. Stove takes it for granted that Popper's

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<sup>2</sup> 'The Idea of Truth and the Problem of the Empirical Character of Scientific Theories', delivered in the International Congress for the Philosophy of Science, at Stanford, California, in August, 1960.

theory of testing is a preliminary to his theory of corroboration, and that this latter theory is a solution to the problems: how do we know and what should we believe? But Popper tries to solve the problem: how do we learn? His answer is: by criticizing our errors. The idea that anything we say can be a subject for a critical examination is the core of Popper's philosophical attitude. Mr. Stove views Popper's recommendation of the critical attitude as a part of his theory of corroboration, and he tries to see whether it is a necessary or an eliminable part of it. He is thus putting the cart before the horse. Popper takes the critical attitude as fundamental. Corroboration, according to him, is one sort of happening in the history of science which results from this attitude and to which, in turn, this attitude should be applied. Mr. Stove takes it for granted that, to Popper, a corroborated theory is corroborated because it is true or likely to be true or credible. As I understand it, Popper's philosophy contains the idea that we should take notice of a well-corroborated theory and try to explain the fact that it was corroborated—and a variety of explanations may be available, each of which should be critically examined. Undoubtedly, Popper's philosophy is connected with a long-standing tradition; but it is the critical tradition of Galileo and Boyle, of Kant and Whewell, rather than the inductivist tradition of Bacon, Newton, and Mill.

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### THREE QUESTIONS FOR PRIOR ON TIME

By JOHN KING-FARLOW

Professor A. N. Prior's views on Time are as thought-provoking as anyone's since McTaggart claimed to unravel the contradictions of temporal talk. To be provoked, however, is not to be convinced and I should like to add three questions to criticisms already advanced over *Time and Modality*.

(I) *Would Prior's ambition eliminate Determinism?* Prior begins his book by confessing to "a hankering for well constructed theories which much contemporary philosophy fails to satisfy".<sup>1</sup> The kind of well constructed theory he has centrally in mind is a logical system in which tense operators perform like Lukasiewicz's modal operators in such a way that, together with certain rules of inference, axioms and truth values, various metaphysical proclivities of Prior's are satisfied. His main concern is

<sup>1</sup> *T. & M.*, preface, p. vii.

to bend logic to "bring out the logical asymmetry between past and future which serious indeterminism seems to demand".<sup>2</sup> He would formalise the view "that from the fact that there is a sea-battle going on it does not follow that there was *going to be one*, though it does follow that there *will have been one*".<sup>3</sup>

Thus Prior's first and intensest motivation for adventuring with symbols is his desire that logic should prejudge the determinist-indeterminist issue. Commenting on this desire, L. J. Cohen has written: "Ordinary logic with its timeless truth evaluations is quite uncommitted to either side. Neither 'Any point instant belonging to a day subsequent to the present one is necessarily occupied by whatever occupies it', nor its contradictory, is a thesis of any familiar text book system. If tense logic tends to beg the question in favour of indeterminism, that is hardly a reason for calling it 'good logic' even if it is good physics or metaphysics".<sup>4</sup> In one way Cohen's criticism goes too far and in another way not nearly far enough. If a metaphysician so bends logic or language as to make it reflect his metaphysical bias, then the finished article *is*, by his canons, a good logic or a good language. But Prior is not merely mistaken in assuming that tense logic is a *necessary condition* for satisfying the demands of serious indeterminism. He is further mistaken in thinking that setting up such a logic supplies anything like a *sufficient condition*.

As to determinism, in any of the usual senses, I certainly agree with Cohen, Donald Williams<sup>5</sup> and others that standard quantification theories are agreeably neutral. On the other hand, whether or not we believe in the existence of timeless or temporal truths about the future is not a question the negative answer to which qualifies us to be called indeterminists, in the appropriate sense of defenders of free will. (That sense of indeterminism is presumably what Prior's references to Aristotle's sea-battle indicate.<sup>6</sup>) Suppose, like C. D. Broad in *Scientific Thought*, I refuse to call any proposition true, false, true-or-false, or even a proposition unless it corresponds or fails to correspond to a suitable past or present fact. This affords me a magnificent asymmetry of Space and Time, perhaps, yet it does not prevent my believing that there is no such thing as human choice, that all present and past facts of human behaviour show distressing regularities no less mechanical than those of brute beasts or falling bodies. It might be suggested that restricting the indeterminate value to

<sup>2</sup> P. 94.

<sup>3</sup> P. 95.

<sup>4</sup> *Philosophical Quarterly*, 1958, p. 270.

<sup>5</sup> *Journal of Philosophy*, 1951, pp. 457 ff.

<sup>6</sup> Cf. *De Interpretatione*, IX.

future propositions about human behaviour is what serious indeterminism demands. This ill fits Prior's willingness to forsake the existence of "facts directly about" *any* objects which do not yet exist. We might move a shade closer to reflecting indeterminism if there were an asymmetry between reference to human and to non-human future objects, but Prior does not provide this. Even so a neuter value concerning future human behaviour could equally well reflect belief (a) that man is utterly unpredictable (hence *not* free, cf. Hume) or (b) that the continued existence of men is too precarious for one to be committed to any future assertion about them. So my first question is: why does tense logic reflect "serious indeterminism" any more than determinism is enforced by timeless truths?

(II) *Do Prior's justifying remarks about reference to what is future support or merely follow from his metaphysical view of Time?* In his (seventh) chapter on Common Noun Logic and at earlier stages Prior is magnanimous about dispensing with the reference resources we have in ordinary language and quantification theory. This is in spite of his campaign promises to respect some basic intuitions of the former and not to tamper with the latter. He writes in self-justification at an early stage: "I am a little uncomfortable about this view that we cannot properly name objects which have ceased to exist, like Bucephalus; but I do not see any way of avoiding it—if we say that we *can* properly name them . . . we are exposed to all the difficulties which were shown earlier to arise with the general theory that there are non-existent objects. Instinctively, all the same, we are happier about granting that we cannot properly name, and there are no facts directly about, objects which *do not yet exist*".<sup>7</sup> Prior goes on to invoke in his support the "very powerful" arguments which Professor Gilbert Ryle raised in *Dilemmas* against naming or referring to the non-existent.<sup>8</sup>

Turning to these "very powerful arguments" of Ryle's, we meet a rather odd inference pattern heavily coated with bluff.<sup>9</sup> We are offered the obvious premise that we "can never point to or 'name' a particular happening and say of it 'This happening was averted'"—obvious, that is, in the sense that talking *about* what did not happen is not entirely like talking *about* what did happen. In the same way, Ryle says plausibly enough, if the Waterloo of 1814 had not been fought or the present Ryle not been born, then there would be no such event or person for historians to describe: certainly there is a sense of *describe*

<sup>7</sup> *T. & M.*, p. 33.

<sup>8</sup> *Pp.* 33-34.

<sup>9</sup> v. *Dilemmas*, pp. 24-27.

appropriate to Ryle's claim. However, Ryle moves on confidently to put *what has not yet occurred* on a logical par with *what did not occur* at some specified past time: in other words we are expected to bracket what is not yet the case with what now can never be the case, as if they were of the same order of substantiality. Why the latter should be so like the former from a referential point of view we are never told. Why indeed should the potential and perhaps all but certain stand in the same sort of relation to the referrer as the now utterly impossible non-entity? The metaphysical view which would lead us to hold this is going to involve Broad's asymmetry of Space and Time, whereby to be present is "simply to precede *nothing*". Ryle like Broad insists on a radical difference between prophecies and chronicles. There is a Broadian ring about Ryle's assertion that no prophet, however vivid and accurate, could "get the future events themselves for the heroes and heroines of his story". Ryle talks throughout as if an examination of our common-sense ways of talking makes it impossible to differ with him, but in the case of this last extravagant metaphor no appropriate common-sensible sense of *get* is clear.

It is this crude metaphor of *getting* which more than anything else suggests that Ryle has presupposed Broad's by no means universally acceptable picture of history. Here a pile of hard specious-present-sized events—which, having already become, we can really get hold of to refer to—are opposed to the practically ineffable vacuum of what is not yet. Similarly, Prior in his paper 'Time after Time'<sup>10</sup> baulks at Pears' talk of the logician's truths as timeless shadows set in a symmetrical Heaven. He prefers to think of events as casting their shadows over what will have become after them, shadows that lengthen with the passage of time. On a rival metaphysical view, like that of Williams, who views the totality of events as spread out *sub specie aeternitatis* in the dimensions of Space and Time alike, we get a symmetry which would make us want to interpret matters of reference very differently. An odder view might make us want to restrict the possibility of *direct* reference to things present and future. How ordinary language could help us to decide between such rival metaphysicians and their accounts of reference is obscure; certainly Ryle and Prior make it all no clearer. So my second question runs: are not Prior's and Ryle's parsimonies in the matter of future reference merely consequences of, not justifying reasons for, their questionable metaphysics of Time?

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<sup>10</sup> *Mind*, 1958, pp. 244-46.

(III) *In so far as Time can be made logically special, cannot Space and Individuals be made special, too?* In a long section of appendix Prior tries to show that there is something special about Time.<sup>11</sup> Constructing some ingenious place-logical and time-logical formulae he argues that we can equate an expression meaning "it is the case  $m$  miles to the left that it is the case  $n$  miles to the left that  $f$ " with another expression in which a symbol represents the algebraic sum of  $m$  and  $n$  or, with a wide range of directions, the vector sum. But, he insists, there is no such analogy in the case of time-logical formulae: "for even if it was the case  $m$  days ago that  $p$ , it might not have been true  $m$  plus  $n$  days ago that it was going to be the case  $n$  days later that  $p$ . For  $m$  plus  $n$  days ago the issue might have been indeterminate". No auxiliary symbol parallel to the one in place-logic can obviate this.<sup>12</sup>

We have already rejected the thesis that we must bring in a neuter value to save future contingencies: Prior's alleged need for special values relative to Time is specious. A neuter value in the case of futures yields not Indeterminism, but something like the unreality of the future, a very different metaphysical position. Williams has retorted to Broad that we might equally well posit the tragic unreality of the past. This view could equally well be represented—not proven—in Prior's Time-logic by assigning neuter values to all formulae about the past. Again, a man walking ever forwards along a straight line might equally well believe in the utter nothingness of all he had left behind. In a backwards-forwards logic he could represent—not prove—his thesis by assigning the special neuter value to all formulae about the regions behind. Again, Prior tells us, though he does not argue in such detail, that there is something special about times as opposed to individuals. But a Platonist could represent the ontological hierarchy of the Line parable in *Republic* Book VI by assigning an ascending order of truth-values to formulae mentioning the ascending order of *gignomena* and *onta*. So my final question runs: surely the only restriction on making times, places or individuals logically special is that we must not make them all special at once, or else none of them will be special?

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<sup>11</sup> *T. & M.*, pp. 117-121.

<sup>12</sup> v. pp. 119-120.

## A POUND IS A POUND IS A POUND

By RONALD J. BUTLER

The logical tangle of the theory of Forms is sometimes met by the suggestion that the theory is to be conveyed not by precise description, but by analogies and allegories and equivocal meanings. It is in this spirit that Mr. Geach has suggested that the Forms are paradigms, comparable to standard weights and measures.<sup>1</sup> The suggestion warrants a hearing not because the logically incomprehensible can become less incomprehensible by a spate of analogies and allegories and equivocations, but rather because the incomprehensibility of the Forms can be thrown into sharper focus by showing the respects in which the analogy fails.

The analogy was introduced in part to elucidate what Professor Vlastos had called the *self-predication assumption* in the Third Man Argument. Although I do not agree that Plato there tacitly assumed that every Form is an example of itself, at least it can be agreed that Plato at some time thought certain Forms to be examples of themselves. The only ones he explicitly mentions are the Just, the Holy and the Beautiful. There is indeed a class of Forms which must have this character, namely the class of Forms of whatever is characteristic of the Forms. I think it not implausible that the three clear-cut cases which Plato mentions were all thought by him, at one time or another, to be members of this class. For example, since all of the Forms are divine and immortal (*Phaedo* 81a), and the ultimate goal is to contemplate the Forms eternally, what else could be holy if the Holy is not holy? And again, the culmination of Diotima's speech is that the beauty in every Form is one and the same (*Symposium* 210ab, 212a), so that as a matter of course the Beautiful is itself beautiful.

Geach's analogy, however, was introduced not to elucidate the Forms of characters, but rather the Forms of objects. In point of fact Plato never gives a clear-cut case of the Form of an object as an example of itself. He does not say, nor does he *imply*, that the Bed is a bed,<sup>2</sup> or that the Circle is a circle. Many passages have been cited in support of the contention that Plato thought of Forms other than those explicitly mentioned as being examples of themselves; but I find none of these other

<sup>1</sup> P. T. Geach, "The Third Man Again", *The Philosophical Review*, Vol. 65 (1956), p. 74. All page references in the text are to this article and to Professor Vlastos's reply, "Postscript to the Third Man", in the same issue of *The Philosophical Review*.

<sup>2</sup> An analysis of Plato's argument at *Republic* 597c is contained in "The Measure and Weight of the Third Man", to be published in *Mind*.

alleged cases at all convincing. There is some doubt, therefore, as to whether the analogy has any application—so far as “the self-predication assumption” is concerned.

Yet Geach means the analogy to elucidate not only this assumption, but also the relation of other things which are not Forms to the Forms in which they participate. “The bed in my bedroom is to the Bed, not as a thing to an attribute or characteristic, but rather as a pound weight or yard measure in a shop to the standard pound or yard” (p. 74). The analogy must therefore be examined in the light of both claims.

Initially Geach is doubtful as to whether “The standard pound weighs a pound” is admissible at all. On the ground that one cannot measure a standard against itself, he is *tempted* to say, but does not say, that such statements are plain absurdities (p. 74). This double line is developed in the penultimate paragraph, for there he says that the standard pound weighs a pound no matter what it weighs, only to add that “just because there is a sense in which the standard pound must be a pound, there is a sense in which it cannot be a pound—not as other weights can be” (pp. 81-82). At this point it is said that one can predicate “... is a pound” of the standard pound and the many pounds only analogously: to which it must be replied that the use of “analogous” in regard to meanings seems to have admitted of no precise explication.

In the quest for precision Vlastos seeks instead the exact sense in which the one use of “... is a pound” is derivative from the other. The standard pound is said “to have the same weight as that of objects which have been (or, can be) found to weigh the same as the Standard Pound” (p. 88). It is on account of this comparison with things that weigh a pound in the ordinary sense of weighing the same as the standard pound, that the standard pound itself is said to weigh a pound derivatively.

Suppose that Mrs. John Doe is handed a pat of butter and asked how much it weighs. After some deliberation she says, “It has the same weight as that of objects which have been (or, can be) found to weigh the same as the standard pound”. Would she not be saying, in her tortuous way, that this pat of butter *weighs a pound*? Suppose instead that Mrs. Doe is handed the object which is said to be the standard pound: could she not give precisely the same answer? This remains true in cases wherein one fails to recognise the object as the standard pound, and in cases wherein it is known to be the standard pound. The latter situation may seldom if ever arise, and if it did one would be inclined to say that of course *that* object weighs a pound. The question “Does that object weigh a pound?” when it just happens

to be the standard pound is not felt to be at all odd: it can in principle be asked of any object which one might lay hands upon. One might even say, "This object weighs a pound, and it might even be the standard pound. Perhaps the Scottish Nationalists left it here for us to find". The other question, "Does the standard pound weigh a pound?" is felt to be more odd, not because it is wrong, but because when the object is specified as the standard pound the question requires a special context. When given that context the context seems odd because it is so precious: but the question in that context no longer rings oddly.

Geach's initial qualms arose over the notion that you cannot measure a standard *against* itself. In every case except one, to say that something is a pound might be taken to mean that *if* the object in question were weighed against the standard pound, then it would be found to weigh the same as the standard pound. But it is a mistake to think that the object which is called the standard pound cannot satisfy the condition laid down in the antecedent simply because it is the standard pound. The reason why the condition laid down in the antecedent cannot be satisfied is because the relation of *weighing against* is irreflexive. "If the pat of butter from Minnesota is weighed against the pat of butter I bought yesterday, it will be found to weigh the same as the pat of butter I bought yesterday" is a perfectly good sentence, provided that the pat of butter I bought yesterday is *not* the pat of butter from Minnesota.

The expression ". . . weighs a pound" can be explicated in terms of the relation of *weighing the same as*, without reference to the relation *weighing against*. Imagine a community which did not use the principle of the balance as the immediate and obvious means of measuring weight, but which had at its disposal other means for telling when two objects weighed the same, or when an object weighed the same on different occasions: then the difficulty of weighing the standard pound against itself would not arise. That is to say, the notion of *weighing against* is dispensable in a way in which the notion of *weighing the same as* is not.

Suppose that the object referred to as the standard pound—let us call it the *SP* in order to distinguish it from the concept of the standard pound—has been kept for a long time in a glass case at a constant temperature. When taken out and tested it is found no longer to weigh a precise pound, there being a uniform discrepancy in all the tests. Imagine the flurry in the world of science, the panic in the world of commerce, the problems in the world of legal enactment. But none of this would cause the standard pound to be abandoned: too much is at stake for there to be any immediate change. True, the *SP* would be abandoned,

since it would no longer accurately represent the standard pound; and to replace the late *SP* would be an engineering problem of the first magnitude. In general, however, standard weights and measures do not exist in isolation: the standard pound is so many grammes, the standard metre is so many inches, the standard litre of  $H_2O$  weighs so many pounds and no more. Standard weights and measures and volumes dovetail together so tightly that it is not true to say that the *SP* weighs a pound no matter what it weighs. The coherence of standards stabilises the standard pound far more than does the *SP*, which could at any moment be detonated out of existence. By virtue of the dovetailing of quite different standards, the phrase "the standard pound" has liaisons with other expressions in the language of such a kind that we can readily verify if the *SP* truly represents the standard pound.

The *SP* has weight, but things having logical liaisons have no weight. To say that the standard pound *weighs* a pound is as absurd as to say that the number three weighs a pound, or that the number three is three things. But that the standard pound *is* a pound is as tautologous as that the number three is a number. Spelled out thus it sounds a little strained: it is far more usual to say without ado that three is a number, or that a pound is a pound.

Could the relation between the things in the world of Becoming and the Forms be like that between the *SP* and the standard pound? If this were so it would need to be assumed that the Forms are conceptual in the way in which the standard pound is conceptual, instead of being things of beauty and objects of reverence. In raising questions about whether the Forms are purely conceptual and can be predicated of themselves a confusion has arisen between "the Form, the Master" (or, more simply, "the Master") which is a referring expression and the Master which, being a Form, does not refer. We can ask of the former whether it can be predicated of itself, but in the case of the latter this question is inapposite. And we can ask of the latter whether it is an example of itself, but in the case of the former this question is inapposite. The standard pound is determined by the manner in which it is specified in statutes, rulings, legal laws and scientific laws; but the Master is not and could not be determined in this manner. Whatever the Master be, it was originally conceived by Plato as a non-temporal and immutable entity in the world of Being. In contrast the Bureau of Standards strives for consistency in meticulously checking standards in the world of Becoming which are intended to endure: but bureaucrats know only too well that the standards could for expediency be changed.

Wherein lies the merit of Geach's analogy? Its value, in so

far as it has any value, lies in focusing attention upon the ideal or unique character of the Forms, upon the claim that the things around us do not measure up to their Forms. It becomes devalued upon discovering that there may be two distinguishable things, not one, that both of these things could change, and that the precise relation between ordinary pounds and either of these things throws no light at all upon the precise relation between a bed and the Bed. So far as "the self-predication assumption" is concerned, it must be reiterated firmly that the Forms, like the *SP*, not being expressions, are not self-predicating expressions.

It is clear that when Plato said that the Beautiful is beautiful he was intending to make a non-tautological statement about an entity in the world of Being. In contrast, the concept of the standard pound, while being non-temporal, can be predicated of itself only tautologically. And the object called the *SP* can be regarded as an example of itself in that any pound weighs a pound; but this, while being non-tautological, has been shown to be not the appropriate parallel.

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## REVIEW

THE LOGIC OF SOCIAL ENQUIRY. By Quentin Gibson. London, Routledge and Kegan Paul, 1960. 214 p. 24s. (U.K.).

Much of the discussion and controversy concerning what it is that social enquirers are doing is frequently muddled by cross-purposes, *ignoratio elenchi*, concealed ambiguity, and failure to make explicit the presuppositions made by the participants. Much of it is sterile because of the failure to disentangle separate issues or, where they have been disentangled, to be as precise as possible in defining them. Professor Gibson's book is concerned with disentangling and precisely defining the issues in the controversy about the nature of social enquiry. But, further, it defends a view of social enquiry as scientific and gives some account of the nature of social enquiries as systematic bodies of knowledge, based on a "methodological individualism".

"The controversy", he says (p. 1), "has arisen . . . out of reflection on differing presuppositions about method which have been implied or thought to be implied in the conduct of these enquiries and out of the differing expectations which ordinary people have about the character and extent of their results". The book is in two parts. Part One (Chs. I-VII) is entitled "Anti-Scientific Views about Social Enquiry" and it examines some of these main differing presuppositions about method. Part Two (Chs. VIII-XVI) is entitled "The Logical Peculiarities of Social Enquiry" and it examines the character and extent of the results that can be expected from social enquiry. The material is very well arranged, and the book is a sustained and coherent argument. I repeatedly found that what I had noted as apparent deficiencies in the argument in early chapters were taken up and fully discussed later in appropriate places.

In Part One a set of claims by which it has been thought to show that social enquiries cannot satisfy one or other of the conditions necessary for any enquiry to be scientific is examined. There have been quite crude attempts to deny the scientific character of social enquiries, as, for example, when a science has been narrowly defined as any enquiry employing the technical procedures of physics. Such as these have not been discussed, and justifiably so, since so large a failure to accord with historical usage would rule out much more than social enquiry. Instead, the proposal (p. 3) is to adopt the definition of a scientific procedure as any procedure that employs each of the following: abstraction, generality, reliance on empirical evidence, ethical neutrality, and objectivity. The argument of the first part is that social enquiries

are not, through any peculiarities either of the subject-matter or the enquirers themselves, unable properly to employ such procedures.

Claims in writings on social theory about the uniqueness of social phenomena, their variety, and the rapidity of social change, about the inefficacy of empirical procedures, the ultimate impossibility of positive, factual, value-free conclusions, and the impossibility of generalising are frequently made in the most tantalising of obscure ways. Chapters II-VI provide a very clear statement of such issues and, if heeded, should do much to dispel some of the woolliness that has hindered discussion of the difficult topic of the status of social enquiry. It is not likely that the book will be regarded as resolving any of the crucial issues, since these are, or are closely related to, matters of continuing philosophical dispute. It has the virtue, however, of showing what issues are worth discussing, and of discussing them in a careful and detailed way.

There are many matters of general philosophical interest raised. In the two chapters on "The Criticism of Generality" (Ch. III, "... Freedom and Change", and Ch. IV, "... Purposes and Reasons"), where the arguments against the use of general statements in social enquiry are examined, such topics as free-will, motives, reasons, purposes and intentions are discussed as providing possible explanations of social affairs which can avoid generalisation. It would be an unreasonable overburdening of the book to try to argue out in detail each of these, although the author makes his own position clear and succeeds in bringing out many of the confusions that there are in discussions of them. For most of these topics, the discussion is appropriate, but occasionally a little more exposition and discussion would have helped. In his discussion of indeterminism, for example, he says (f.n., p. 22) that "it is impossible to enter into the subtleties of this controversy here". It is likely, however, that anyone who wanted to urge the occurrence of free-will against the possibility of general explanations in human affairs would want to enter into the subtleties of the argument. Such a person, too, would argue that the social psychologist's speculations in the example on p. 23, which "might lead him to apply to this case not only rules of chance but universal laws of human behaviour", would be utterly idle. The argument from free-will is so frequently urged in this context that a more detailed statement of this issue, and a more explicit justification of the author's own position, would have been an advantage.

Objectivity has a special place in the list of defining characteristics already mentioned. According to the argument of the

book, the upholders of "anti-scientific views" can admit that social enquirers do engage in the first four activities. They occur in social enquiries, but they are ineffective: there are effective, alternative procedures, and it is only by adopting these that important knowledge of human and social affairs can be established (pp. 3, 74). "But in the case of objectivity the position is reversed. They do not wish to criticize a procedure in which objectivity is maintained—they wish rather to deny that it is possible. They recognize that it is desirable for social enquirers to take account of evidence, but they assert that the general circumstances of social enquiry are such as to prevent even the most intelligent from using the evidence at their disposal" (p. 74).

Objectivity is important also in Gibson's argument. In asking whether social enquiry is objective, he explicitly considers the theorist. He considers the question, "Can social enquirers be objective?". He could also have explicitly considered the theory. Social theories are sets of statements, standing in specific logical relations to one another. It is possible to ask whether such theories can be objective and mean by this something quite different from asking whether theorists are objective. What one would be asking about would be, for example, such matters as the kinds of statements that occur in the theory, the possible evidence for them and its accessibility, and the logical relations that hold among them. These matters are discussed in the book, but not directly in relation to objectivity. The topic of the kinds of statements that are possible in social theory and of their logical relations is one that receives considerable attention. The most important account of these is that of "tendency statements" and their function in systematic theory (Chapter XIII). Other statements considered are ethical or value statements and those statements asserting the occurrence of psychological experiences or states of mind.

Even if it were the case that social scientists cannot avoid value-judgments, by which I mean the making of ethical statements, rather than being influenced by ethical considerations in arriving at conclusions or in considering evidence (about both of which Gibson is extremely lucid), only certain ethical theories pose problems for the discussion of objectivity. These are those subjectivist theories that claim ethical terms to be reducible to terms describing subjective feelings or states of mind, and those that claim ethical terms to be irreducible. Theories of the second kind pose the problem of whether certain propositions of social theory require methods of observation other than regular empirical methods. Theories of the first kind, along with those other state-

ments asserting subjective experiences, raise questions concerning the accessibility of evidence.

On the last of these Gibson supports introspection and denies the behaviourist and Rylean account (p. 48 and f.n. 1), i.e., he claims that the empirical evidence about the states of mind of people other than ourselves cannot be anything but indirect. He argues (p. 49) in support that "the position of the social enquirer is in this respect similar to that of other enquirers" and instances the evidence for past events and the evidence for theoretical entities, such as "light-waves, electrons or genes". Physicists and biologists, he says (p. 49, f.n. 1) leave the interpretation of these to philosophers "and there is no reason why social scientists should not do the same".

Now, there is an important difference of principle here, namely that, in the case of the theoretical entities of physics and biology, no one is in principle in a privileged position in relation to the evidence, whereas, in the case of states of mind, it is claimed that in the last resort someone has privileged access to some of the evidence. Indeed, he says (p. 49) that "the empirical knowledge we have of the thoughts and feelings of others depends, in short, on generalization from our own experience". The difficulties of such generalization are not underestimated, but it seems to me that some of the arguments in Chapter V against "sympathetic understanding" and "intuitive insight" as effective, alternative procedures to empirical investigation might apply equally well to the *ultimately* private nature of the evidence for statements about a person's states of mind.

An objective procedure is described (p. 3) as one "in which no-one is prevented by the general circumstances of the enquiry from basing his statements on a consideration of the evidence". The denial of the possibility of objectivity in social enquiry is the contrary of this, that anyone is prevented by the general circumstances of such enquiry from basing his statements on a consideration of the evidence. The circumstances considered relevant are those of motives, custom, and social situation (p. 77 ff.). The discussion of these and related points in Chapter VII, "The Denial of Objectivity", is very good, but one part of the argument (which is important, because it is taken up and developed in Chapter XIV, "The Assumption of Rationality") seems to be deficient in not examining the notion of "evidence" sufficiently.

The following is a summary of the argument of Chapter VII, pp. 73-76. To deny that a man is objective is to assert that he is prejudiced, biased, protecting special interests and the like. It is a form of *argumentum ad hominem*, and is essentially a

means of attacking someone for the way in which he comes to hold his beliefs. When it is directed against the whole body of social enquirers, it is a means of attacking social enquirers as such for the ways in which they come to hold their beliefs. Depending as it does, then, upon causal considerations or considerations of origin, it has been, normally implicitly, taken to be irrelevant to the *logic* of social enquiry, where the important questions are those of beliefs and the evidence for them. It is not, however, irrelevant for this reason: that, in relation to beliefs, evidence has a dual role. It is at once the logical justification of the belief, and the cause, or part of the cause, of the belief, at least in those cases where the belief is in some degree rational. For this reason, the possibility of objectivity, the possibility of arriving at beliefs, not under the influence of custom or one's social situation, but by taking account of the evidence, needs to be shown.

I agree with the conclusions arrived at concerning the possibility of objectivity of enquirers, but think that their support requires more emphasis upon controversy, mentioned only in the last paragraph: in other words, upon the conditions of dispute, challenge, and, especially, the possibility of *agreement* among social enquirers. In this context, objectivity or the lack of it would be demonstrated in such things as meeting requests for elucidation of claims, for considering suggested counter-evidence and so on. Relevant to this would be the principles of explication, the principles used in assessing evidence and in determining what is to be admitted as evidence.

I should have liked to see in the book some general discussion of the admissibility of evidence in social enquiry. He has much to say about evidence, but it seems to me that he is not perfectly clear about two quite different kinds of thing. When he discusses the evidence for beliefs, he speaks sometimes of evidence in an absolute, non-rational or non-logical sense, as, roughly, what there is to be discovered. It is in this sense that evidence is used in the discussion of the denial of objectivity. Showing that the evidence can be taken account of is showing that what is going on in society can be discovered. The upholder of the anti-scientific view denies that what is going on in society can be discovered directly and says that it can only ever be observed indirectly through some distorting set of concepts. To deal properly with this would require, however, a general discussion of evidence and Gibson has explicitly restricted himself so far as possible to questions of scientific method in social enquiry.

Evidence in the rational or logical sense consists in propositions standing in certain logical relations to other propositions

and having a certain status among enquirers, namely of being accepted (or believed) in one sense without evidence. When, for example, a social theorist is charged with bias, i.e., with not having considered the evidence, he may meet the charge by arguing that "the evidence" is not really evidence. I should have liked to see some amplification of this sort of issue in the book. This, however, may be quibbling.

There are many other topics of importance in the book that I have not mentioned. Among these are very good discussions of the nature and function of general statements in social enquiry, historical explanations, and the relation between psychology and social enquiry. There can be no doubt that the book is an important contribution to the discussion of matters that do not often receive the careful and rigorous attention that Gibson gives them.

I have noted only a few minor misprints that do not affect the argument.

R. S. WALTERS.

## BOOKS RECEIVED

(Inclusion in this list neither guarantees nor precludes later review.)

ARMSTRONG, D. M. Berkeley's theory of vision; a critical examination of Bishop Berkeley's *Essay towards a new theory of vision*. Melbourne University Press, 1961. xi, 106 p. 26s. (Australian).

Argues that Berkeley's *Essay* is more important as "a heterodox view of the general structure of the reality revealed to the senses of sight and touch" than as a half-way house to immaterialism. The author is, however, critical of Berkeley's views on the relation between sight and touch.

BACON, Francis. The new organon, and related writings; edited, with an introduction, by Fulton H. Anderson. New York, Liberal Arts Press, 1960. xli, 292 p. \$1.35 (paper covers).

BAHM, Archie J. Types of intuition. (University of New Mexico Publications in Social Sciences and Philosophy, number 3.) Albuquerque University of New Mexico Press, 1961. \$1.25 (paper covers).

"The present study demonstrates how intuition is presupposed in all knowing . . . introduces the theory of knowledge of organicism, a new integrative type of philosophy." Blurb.

BLANCO, Alejandro Diez. La filosofia y sus problemas. Barcelona. Editorial Scientia 1960. 217 p. 200 pesetas.

A critical exposition, for beginners, of leading philosophical theories. Deals with epistemology, philosophy of science, ontology, philosophy of religion, and ethics.

CRAIG, Hardin. New lamps for old. Oxford, Blackwell, 1960. viii, 244 p. 4s. 6d. (Australian).

Rejecting positivism, the author tries to find in "the philosophy of relativity with its naturalistic theory of cognition" the germs of "a new renaissance" in "humanistic knowledge and humanistic relativity".

DATE, V. H., ed. Vedānta explained: Samkara's commentary on the Brahmasūtras, volume 2; with, New light on the philosophy of Samkara. Bombay, Bookseller's Publishing Co., 1959. viii, 552 p. Rs. 27.50.

EDEL, Abraham. Science and the structure of ethics. (International Encyclopedia of Unified Science, vol. 2, No. 3.) University of Chicago Press, 1961. iv, 101 p. \$2.25.

Discusses scientific method in ethics, and concludes that using it is "genuinely possible, with the extent of its practicability dependent on the extent of determinateness found in the human field".

FEIBLEMAN, James K. An introduction to Peirce's philosophy. London, Allen & Unwin, 1960. xx, 503 p. 50s. (U.K.).

"This book has two aims . . . to offer an introduction to the general philosophy of Charles S. Peirce . . . [and] to exhibit the system which seems to be inherent in Peirce's philosophy."—Preface.

- GARNETT, A. Campbell. *Contemporary thought and the return to religion*. Lexington, Kentucky, College of the Bible, 1960. 99 p. \$2 (paper covers).
- GENTILE, Giovanni. *Genesis and structure of society*; translated by H. S. Harris. Urbana, University of Illinois Press, 1960. 228 p. \$4.50.  
Gentile's last book, written in 1943 and posthumously published.
- GILLESPIE, Charles Coulston. *The edge of objectivity; an essay in the history of scientific ideas*. Princeton University Press; Oxford University Press, 1960. x, 562 p. 82s. 6d. (Australian).  
An interpretative history of scientific thought from Galileo to Maxwell.
- HALLDEN, Soren. *True love, true humour and true religion: a semantic study*. (Library of Theoria, No. 6.) Lund, Gleerup; Copenhagen, Munksgaard, 1960. 112 p. 13 Sw. Kr.  
Discusses what is meant by talk of essences ("the essence of religion" etc.) using these three terms as examples.
- HARRIS, H. S. *The social philosophy of Giovanni Gentile*. Urbana, University of Illinois Press, 1960. xii, 387 p. \$5.75.  
"The aim of the present study is to set aside the polemical prejudices arising from Gentile's commitment to a particular political regime, and to consider this theory on its merits as a theory . . . A great gulf always existed between the theory of 'Fascist idealism' and the actual practice of Fascism . . . and . . . 'Fascist idealism' is itself a radical deformation of Gentile's theory."—Preface.
- HEIDEGGER, Martin. *Essays in metaphysics: identity and difference*. New York, Philosophical Library, 1960. 82 p. \$2.75.  
Two lectures published in German in 1957.
- KIMPEL, Ben. *The principles of moral philosophy*. xviii, 234 p. New York, Philosophical Library, 1960. \$3.75.  
An attempt at what the author calls "an empirical moral philosophy"; by which he seems to mean one that will help us to realise worthy moral ideals.
- MINKUS, P. A. *Philosophy of the person*. Oxford, Blackwell, 1960. xv, 95 p. 12s. 6d. (U.K.) (paper covers).  
Doctrines of personal identity in Locke, Hume and Reid, sceptical positions to which they give rise, and their resolution.
- PLATO. *Phaedo*, translated with introduction by R. Hackforth. New York, Liberal Arts Press, 1960. x, 200 p. \$1.25 (paper covers).
- QUINE, Willard Van Orman. *Word and object*. Cambridge, Mass., Technology Press of the Massachusetts Institute of Technology and John Wiley & Sons, 1960. xv, 294 p. \$5.50.  
Discusses "the semantics of objective reference" from the viewpoint of a dispositional theory of meaning.
- RAPHAEL, D. D. *The paradox of tragedy: The Mahlon Powell Lectures [University of Indiana] 1959*. London, Allen & Unwin, 1960. 112 p. 16s. (U.K.).

Discusses the essential nature of tragedy (a conflict between human heroism and impersonal necessity), its connection with religion (the outlook implied is incompatible with Christianity) and with philosophy (Plato was influenced by Greek drama, and, conversely, Sartre or Arthur Miller present themes that are philosophical in at least the broader sense of the word).

**ROUSSEAU**, Jean-Jacques. Politics and the arts; letter to M. D'Alembert on the theatre. Translated, with introduction and notes, by Allan Bloom. Glencoe, Ill., Free Press, 1960. xxxviii, 153 p. \$4. A defence of censorship.

**SARTRE**, Jean-Paul. To freedom condemned; a guide to his philosophy by Justus Strelle, translated and with an introduction by Wade Baskin. New York, Philosophical Library, 1960. 163 p. \$3.

Short extracts from Sartre, arranged under such headings as "Grace", "Facticity", "The Hole". (The hole is something which longs to be filled.)

**SCHNEIDER**, Herbert W. Morals for mankind: the Paul Anthony Brick Lectures, 1960. Columbia, Miss., University of Missouri Press, 1960. xiii, 82 p. \$2.50.

The three lectures deal respectively with conscience, religion and ethics, and social obligations. Popular rather than philosophical.

**SHARMA**, B. N. K. A history of the Dvaita school of Vedānta and its literature; volume 1. Bombay, Booksellers' Publishing Co., 1960. xviii, 372 p. Rs. 17.50.

**SOLMSEN**, Friedrich. Aristotle's system of the physical world; a comparison with his predecessors. (Cornell Studies in Classical Philology, vol. xxxiii.) Ithaca, N.Y., Cornell University Press, 1960. xiv, 468 p. 77s. 6d. (Australian).

The influence of the pre-Socratics, and of Plato, on Aristotle.

**UMEN**, Samuel. The nature of Judaism. New York, Philosophical Library, 1961. xiv, 152 p. \$3.75.

**WELTY**, Eberhard. A handbook of Christian ethics: volume I, Man in society. Freiburg, Herder; London, Nelson. xvi, 395 p. 62s. (Australian).

Roman Catholic doctrine on social theory and practice, in question and answer form, with quotations from papal pronouncements.

**WITTGENSTEIN**, Ludwig. Notebooks 1914-1916, edited by G. H. von Wright and G. E. M. Anscombe. Oxford, Blackwell, 1961. vi, 91 + 91 + 40 p. 52s. 9d. (Australian).

Three note-books of jottings written during the *Tractatus* period, in the German text with an English translation; and two sets of notes, given to Russell and Moore respectively.

**ZIFF**, Paul. Semantic analysis. Ithaca, N.Y., Cornell University Press, 1960. x, 255 p. 55s. (Australian).

Analyses the uses of a particular word ("good") in ordinary language, in order to draw general conclusions about (not ethics but) meaning.

## NOTES AND NEWS

### A.A.P. ANNUAL CONGRESS, 1961

An Annual Congress for 1961 will be held in Canberra from August 21st to August 25th. Accommodation will be available.

The proceedings will open with the Presidential Address on the evening of August 21st. The Annual General Meeting will be held at 5 p.m. on Wednesday, August 23rd, and the dinner will be on that evening.

Further information may be had on application to the Hon. Secretary of the Congress, Dr. Robert Brown, Department of Social Philosophy, Institute of Advanced Studies, Australian National University, Canberra, A.C.T.